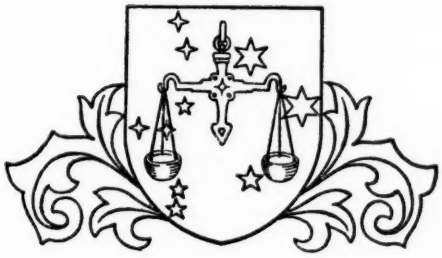
1831  1951

THE ADVANCEMENT OF SCIENCE

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ACKNOWLEDGEMENTS

The portrait of H.R.H. The Duke of Edinburgh is by Baron.

The portraits of Sir David Brunt, Sir Cyril Hinshelwood, Prof. W. B. R. King, Dr. O. J. R. Howarth, Prof. R. G. Hawtrey, Sir Claude Gibb, Prof. H. P. Gilding, Prof. C. A. Mace, Prof. W. Brown, and Sir Hector Hetherington are by Walter Stoneman (J. Russell and Sons).

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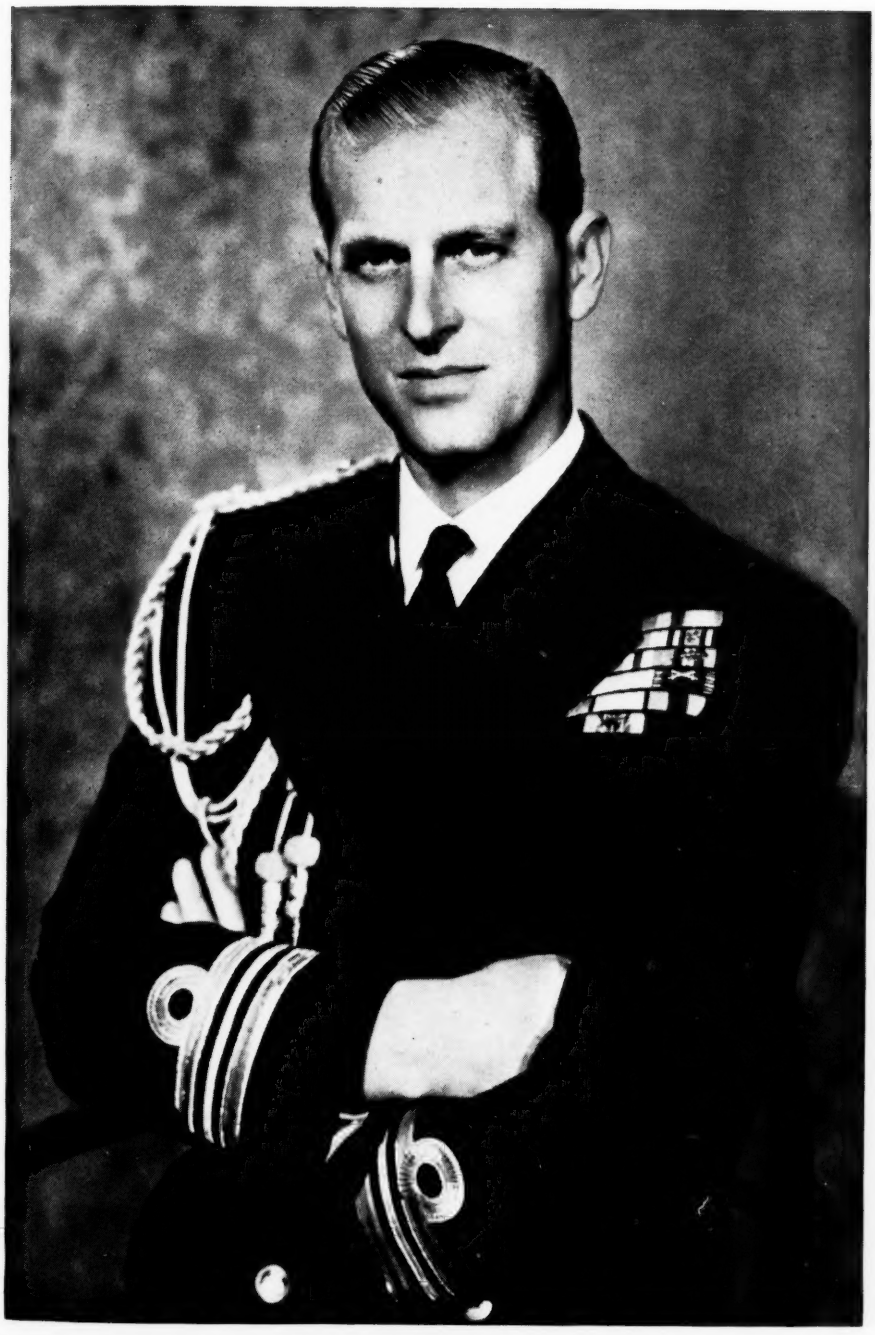

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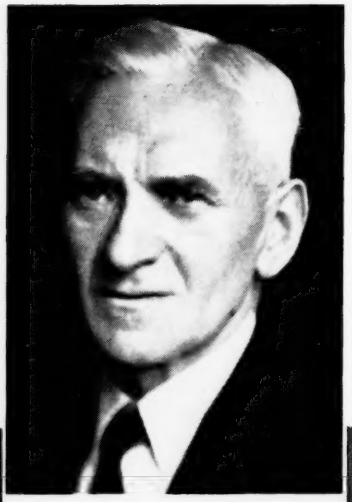

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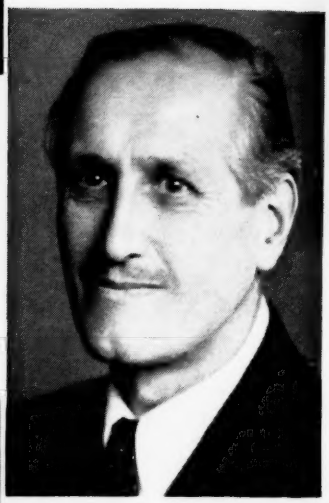

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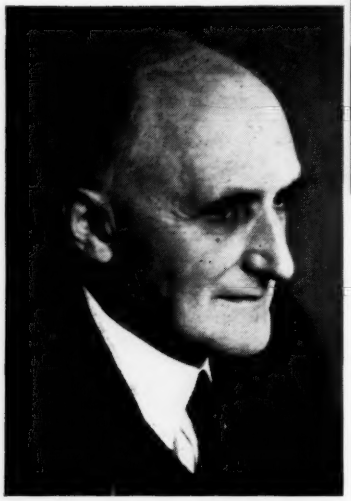

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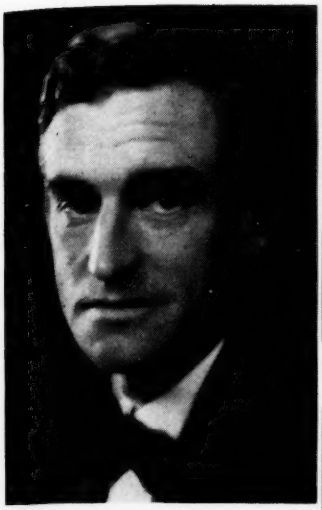

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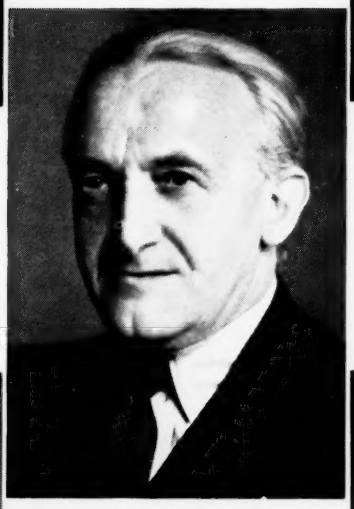

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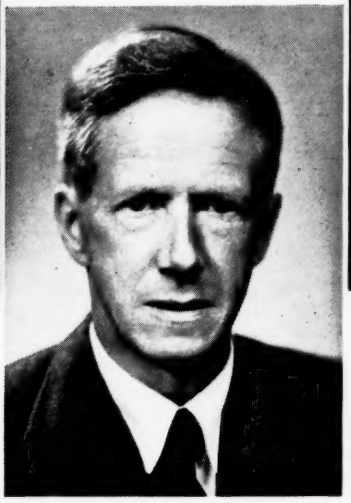

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THE BRITISH CONTRIBUTION TO SCIENCE AND TECHNOLOGY IN THE PAST HUNDRED YEARS

Address by

H.R.H. THE DUKE OF EDINBURGH, K.G., F.R.S.

PRESIDENT OF THE ASSOCIATION

My first and very pleasant duty to-night is to express to you, My Lord Provost, and to you, Mr. Principal, the British Association's deep gratitude for your charming welcome and for the splendid hospitality of the City and University of Edinburgh for this our 113th Annual Meeting. I think I speak for all members, young and old, when I say that we are glad to be back after only thirty years

To mark the opening of this year's Meeting, His Majesty The King has been graciously pleased to send the following message :

> I shall be glad if you will express to the members of the British Association for the Advancement of Science my appreciation of their having once more honoured a member of my family by inviting him to be their President.
>
> I trust that this year's meeting of the Association will further the development of Science for the benefit of mankind throughout the world, and prove an encouragement to all those men and women who are so devotedly working for that end.

On your behalf I propose to reply in these words :

> The Members of the British Association for the Advancement of Science assembled at Edinburgh send Your Majesty their humble duty and their loyal thanks for Your Majesty's Patronage of the Association and your gracious message of encouragement.
>
> I am to assure Your Majesty that the development of Science for the well-being of Your Majesty's Realm and for the general welfare of mankind is the constant object of the Association.
>
> We will strive with all humility and with the grace of God to apply the blessings of scientific and technological improvement to the problems which face us all to-day.

In Aberdeen in 1859 my great-great-grandfather started his address to the British Association with these words :

'Your kind invitation to me to undertake the Office of your President for the ensuing year could not but startle me on its first announcement. The high position which Science occupies, the vast number of distinguished men who labour in her sacred cause, and whose achievements, while spreading innumerable benefits, justly attract the admiration of mankind, contrasted strongly in my mind with the consciousness of my own insignificance in this respect.'

I cannot improve on this to express my own feelings, but, like him, I reflected upon your invitation and came to the conclusion that it is just as an outsider, a layman so to speak, that I can be useful to you and to Science. This very invitation seems to me to demonstrate that Science is not a magic circle and that you wish us to enter your confidence. In return the least I can do is to show our appreciation of the work of scientists and to give you a layman's impression of the march of Science in the last hundred years. I crave your indulgence if I have drawn any false conclusions and I hope that during the meetings which will follow this one, the

experts will take the opportunity to make any corrections.

The Prince Consort had very much less reason to be modest about addressing you than I have, because this year we celebrate the anniversary of the Exhibition which was his greatest achievement, and an event which had an untold value to Science. Let us hope that this year's Festival will be judged a worthy successor and an inspiration for the future. I am proud to pay tribute to this man who saw so clearly the part Science was destined to play in the future of this country, and my address to you to-night is largely the story of the fulfilment of his hopes.

THE STARTING POINT

In a review of British science and technology 1851 is a convenient starting point for two reasons. Firstly, the Exhibition of that year can be regarded as a gigantic stocktaking of the national resources and technical skill. Secondly, because it marked the end of the Industrial Revolution and the conversion of Victorian England to the policy of industrial expansion on which our future still depends. The period as a whole saw the climax of our industrial supremacy and its inevitable decline when countries with greater resources and population learned from us the lessons of the mechanisation of industry. It also covers the birth and growth of the new concepts of modern science.

Social conditions of a hundred years ago were, generally speaking, the outcome of the Industrial Revolution, but with all the traditions of the England of agriculture, cottage industry and small market towns. The population of 20 millions was growing fast but still small compared to our 50 millions of to-day. Education was limited to a minority and was almost entirely classical, so the new profession of engineering had to draw its recruits from a different sphere, that of self-educated men. A new wealthy class was growing up in the commercial world to rival the old aristocracy. There was unbounded optimism about the future and ample scope in commerce and industry to attract all intelligent and enterprising men. The number of poor was on the increase and their conditions were deteriorating because, as yet, no social conscience had grown up to replace the patriarchal responsibility of the landowners and master craftsmen.

In the domestic field, lighting was by candle and oil lamps, cooking and heating by coal or wood in ranges or open fires with the consequent enormous waste of energy. Food had to be fresh or crudely preserved, and thus needed to be produced locally. In health and hygiene the figures speak for themselves. In 1851 the infant death rate was 150 per 1000 living births compared with 25 per 1000 to-day. Anaesthetics, antiseptic surgery, biochemistry, tropical medicine, were all virtually unknown or in their infancy. Psychology had not yet achieved independence from philosophy on the one hand and physiology on the other.

This was the age of the practical engineer and of processes arrived at by intuition born of experience and by trial and error. Technology was concerned with the application of steam power, with metallurgy and the working of metals for various purposes, and with the production of machine tools and precision machinery. Men were already turning their minds to other types of engines and the internal combustion engine was in the process of development.

Scientists, while continuing their search for the secrets of nature, were beginning to turn their attention to exploring the empirical developments of industry. Their numbers as yet were small, the endowments for research were negligible and much of their work was carried out in the watertight compartments of the different sciences. But the seed had been sown and it was not long before scientists and engineers were preparing the way for the great technological harvest of the twentieth century.

THE CONDITIONS

The changes brought about in the lives of men and women in the last hundred years have been greater and more rapid than during any other period in history, and these changes have been almost entirely due to the work of scientists and technologists all over the world. They have not only affected the way of living of all civilised peoples but have also vastly increased our knowledge about ourselves,

the earth we live on and the universe around us. I cannot emphasise too much that the sum total of scientific knowledge and technological progress is an international achievement to which every civilised country has made some contribution.

And now before considering the contribution of the British Commonwealth, I should like to sketch what appear to a layman like myself to have been the main influences on the course of scientific and technical achievement since 1851 and their relation to one another.

The great stimulus of the 1851 Exhibition created a growing interest in technical education and research, followed by a widening of the scientific horizon which was soon to find expression in borderline subjects. For the next fifty years science advanced rapidly, but in most fields there was a wide gap between science and industry. Electricity was an exception and the groundwork was already being laid for the electrical revolution of the Victorian age. Medicine was on the verge of breaking away from mediaeval practice and taking the first steps towards its modern pattern, while British colonial development stimulated the study of tropical disease.

Between 1851 and 1870 practice, in many industries, was ahead of science, and in that period the large number of inventions of the industrial revolution were progressively improved and widely applied. These inventions, which added so much to our industrial production, were mainly the work of British genius. They were of great economic advantage to this country and were quickly exploited commercially. New factories and plants were built to include the very latest ideas, and with the expansion of industry came the demand for more and more new ideas and greater efficiency. This demand was a direct stimulus to technological invention as well as an indirect stimulus to science. We are still struggling with the social results of this vast expansion.

From 1870 to 1890 the highwater mark of British industrial expansion, as compared with other countries, had been reached and the competition of the United States and Europe was just beginning to be felt. But the lack of serious competition hitherto had bred a feeling of over-confidence and satisfaction in the methods and processes employed. The result was a conservative attitude towards technical change and, particularly in the older industries, neglect of scientific research. Accumulation of wealth and the income from foreign investments in any case made the country as a whole less dependent on the efficiency of her industries. Concurrently a subtle change occurred in the type of British exports. So far the products of our machinery, such as rails and rolling stock, had been shipped abroad for immediate use, but now machines themselves were exported to do their work in the factories of Europe and America instead of in Britain. The result of this was to intensify foreign industrial competition between 1890 and 1914, but with the increasing demands from the Colonies the volume of British exports was not greatly affected.

Then came the critical years of the First World War bringing a realisation of the part science must play in the industrial and military strength of the nation. For the first time in history a real attempt was made to enlist the services of science in the war effort and the Department of Scientific and Industrial Research was founded to further the application of science in industry through Government Laboratories and Research Associations.

The effects of these measures appeared clearly in the inter-war years when there was a marked swing of education from classics towards science. Coupled with this the war had directed the attention of many research scientists to practical objectives so that after the war there was a rapid expansion of industrial research. Scientific progress was no longer confined to the work of a few brilliant individuals, but came also from teams of research scientists each working on different parts of the same problem. It was during this period that many new commercial research laboratories grew up, employing scientists to discover new processes and materials connected with their industry as a direct weapon of competition.

The war had also shown a great weakness in our dependence on foreign production for many vital articles, such as dyestuffs, scientific instruments and optical glass, in the manufacture of which

scientific research played an essential part. This weakness was remedied with the help of the Key Industry Import Duties which gave the necessary support and encouragement to the establishment of these industries at home.

It is true that manufacturers in some of the older industries still clung to traditional methods in spite of the pressure of competition from America and other countries. And in this connection it is significant that the history of production engineering after 1890 is almost entirely confined to the United States.

It was however a period of rapid development in Britain. The invention of the internal combustion engine and the pneumatic tyre had opened new branches of industrial engineering, and the demand for fuel for motor cars and aircraft gave birth to the new technology of oil. In the electrical, chemical and aircraft industries, science was fully enlisted in the fields of electronics, synthetic fibres, plastics, aerodynamics and light alloys. Consequently the outbreak of war in 1939 found us in a much stronger position to meet the immense demands it made on all branches of technology for new gadgets, machines and weapons. From the outset science in all its forms and branches was harnessed and completely co-ordinated with the war effort. It was only the intimate partnership of science and engineering with the Staffs of the Fighting Services that enabled us to meet swiftly and effectively the ever-changing menace of total war.

The tremendous demands on our industries had some good after-effects. Once again these demands revealed weaknesses where our industrial capacity was out of date. The realisation of this has initiated comprehensive reconstruction on most modern lines. The almost complete absence of income from our foreign investments has forced us to rely once more on our capacity to make the goods the world requires. Our industry and productivity have shown a wonderful improvement, but there is still a lot more that can be done. The rate at which scientific knowledge is being applied in many industries is too small and too slow. Our physical resources have dwindled, but the intellectual capacity of our scientists and engineers is as great as ever and it is upon their ingenuity that our future prosperity largely depends.

THE CONTRIBUTION

I would now like to make a brief survey of the British contribution to natural knowledge and technology and pay a tribute to some of the great men of science of the last hundred years.

In some branches almost the whole story can be told since one problem after another has been solved by British scientists. In others there are many blanks and gaps where the vital links in the chain were forged abroad. But looking at the whole vast field of abstract and practical science there can be no doubt that during this period the contribution of the British Commonwealth has been of outstanding importance.

Our knowledge of the stars, the heavens, and our place in the universe has increased steadily through the centuries, but since 1851 some of the most important links were supplied by such men as Eddington, Jeans and Milne in their work on mass, luminosity and stellar evolution. Huggins made a great contribution with his application of spectrum analysis to astronomy, and Lockyer's discovery of helium in the sun had a significance far beyond the realms of astrophysics.

Coming nearer to the earth, the work of Abercromby and Shaw on the behaviour of the earth's atmosphere in the troposphere started the scientific study of weather and weather prediction, and Appleton's research into the ionosphere extended this to the upper air.

Chemistry has fascinated man from the earliest times, and vast progress has been made in the last hundred years both in knowledge and theory. Much fresh ground was broken by Crookes by his work on spectra, his discovery of thallium and of ' radiant matter ' known later as cathode rays. Long after everyone was quite sure of the composition of the air, Rayleigh found another ingredient which he called argon and so started the hunt for other inert gases. In organic chemistry both Perkin and Robinson have added enormously to our knowledge of the structure of carbon compounds, and to our power to copy natural products synthetically. The development of X-ray analysis by the

two Braggs, father and son, has given us a means of finding the actual arrangement of the atoms in the molecule and has revealed the accuracy of the chemists' conclusions about the architecture of molecules based on their reactions with one another. This is a most striking example of the power of the theoretical and practical scientist to penetrate nature's secrets.

Going beyond the chemist and his molecules we come to the physicist and the study of even smaller particles. Thomson's discovery of the nature of the electron was the first attack upon the integrity of the atom. Next, thanks to Rutherford's brilliant research and keen intuition, came the nuclear theory which revolutionised our ideas of matter. To prove it, he was the first man to succeed in the transmutation of an element. It is appropriate to mention Moseley's work on the X-ray spectra of the elements, as it already showed such great promise, before he was killed at Gallipoli.

Parallel with this activity in the physical sciences there occurred a technological revolution of even greater scope and variety. The Darbys of Coalbrookdale were the lineal ancestors of Bessemer, Thomas and Siemens, and the whole technology of metals. First cheap cast iron followed by cheap steel, then steel from phosphatic ores, completely changed the materials available to engineers, shipbuilders and architects. Scientific metallurgy can be said to have started when Sorby first applied a microscope to the surface of metals. The way was opened for the investigation of the metallic alloys which came in quick succession from developments in which Hadfield and Rosenhain made outstanding contributions.

It was not long before the possibilities of these new materials were recognised, and the great majority of the mechanical developments of the period were due to new alloys which could withstand higher stresses. But before these materials could be fully used Maudsley and Whitworth had to lay the foundations of production engineering, and Mushet had to do pioneer work in developing tungsten steel as the first high speed cutting tool.

The reciprocating steam engine of the industrial revolution was the main source of power until Parsons invented the steam turbine, which revolutionised large-scale power production on land and sea. But that was not the only source of power to rival the push-and-pull engine. The internal combustion engine, in which Dugald Clerk and Ackroyd-Stuart were among the early pioneers, has proved to be a formidable challenger in many fields. In marine engineering, Froude's work on hull forms and propellers enabled the full benefit of the new prime movers to be reaped at sea.

Here I wish I could mention early British pioneers of motor-vehicles but, as is well known, restrictive legislation drove the development of the motor-car abroad, until the repeal of the speed limit in 1903 gave scope to the genius of Royce, Lanchester and Ricardo. In place of the motor car, however, we have Lawson to thank for the invention of the safety bicycle ; and all wheeled vehicles except those running on rails, owe their rapid development to Dunlop's invention of the pneumatic tyre. The material required for this started the vast natural and synthetic rubber industry, and has made famous the name of Wickham for a brilliant feat of smuggling, when he brought the rubber seeds from Brazil to Kew, from which sprang the rubber plantation industry of the east.

In flying, the names of the pioneers and their feats are legion, and more than in any other mechanical science the development of aerodynamics has been shared by many nations, but Lanchester's vortex theory was one of the stepping stones to powered flight, and the achievement of Alcock and Brown in making the first Atlantic flight in 1919 speaks highly for the tremendous scientific and technological background of flying in this country. Of outstanding importance and consequence was the genius which Mitchell brought to aircraft design, and, more recently, Whittle's pioneer work has given us the lead in jet engine production both for civil and military use.

Following on the immense progress in metallurgy and mechanical engineering, the most far-reaching development of the period has been that of electricity and electronics. Although the key discovery belongs to Faraday in an earlier period,

the second founder of the science is undoubtedly Clerk Maxwell, with his classic treatise on electro-magnetism. The use of electricity for domestic and industrial purposes was helped by Wilde's development of the dynamo and then by Swan's incandescent lamps. Wheatstone and Kelvin pioneered the use of electricity for communication by their work on line and cable telegraphy. Wireless telegraphy soon followed and the work on tuned circuits by Lodge, and Marconi's many brilliant developments made in this country with the General Post Office and the Navy, soon made radio a practical proposition. Heaviside and Appleton made further contributions on the propagation of radio waves. It is interesting to see that the technique of pulse-ranging, which had been used by Appleton and others in their work on the ionosphere, was later developed by Watson-Watt into radar which is now almost indispensable to airmen and seamen all over the world. And here Randall's development of the magnetron for high frequency radar was one of the major contributions to the Allies' equipment for war.

Television has a wide parentage, but Baird's name will always be linked with the first successful pictures.

Another great innovation of this hundred years was the discovery and development of plastic and synthetic materials. The story starts with Parke's discovery of celluloid and Cross and Bevan's manufacture of viscose which gave birth to the rayon industry and the many later types of synthetic fibre. Perkin's mauve, first of the aniline dyes and Kipping's new silicon compounds were, however, disregarded by industry in this country. But we see to-day a change of heart in the development in our industrial laboratories of two new plastics, perspex and polythene with almost an unlimited range of applications in the air, on the ground, and at sea.

The effect all this has had upon the citizen varies naturally with where and how he lives, but basically it has given him reliable light and heat in his home, push button communication with almost any part of the world and home entertainment of a high quality. His transport on land, at sea, and in the air is quick, comfortable and clean. In addition he has a vast range of materials with which to clothe himself and to furnish and embellish his home. Almost more important, these developments have brought about a complete change in his conditions of work.

But if the citizen has benefited, so too has Science from the great array of new techniques that have been invented, and the new tools with which the scientist and technologist can burrow, hack and worry at the growing mountain of problems to be solved.

So far I have dealt with the physical sciences. Now I would like to turn briefly to the biological and psychological sides, which after a slow beginning in this country have made increasingly rapid progress.

The whole field of biological science in this period is overshadowed by the works of Darwin presented in his 'Origin of Species' and 'The Descent of Man.' Nothing has done so much to widen man's thoughts as his conception of evolution as the great law controlling living things, 'that progress comes from unceasing competition, through increasing selection and rejection.'

In the basic study of living things some of the most important contributions from this country were the pioneer work of Francis Galton and William Bateson in the field of heredity, Sherrington's work on the integrative action of the nervous system, and Dale's and Adrian's contributions to our knowledge of the transmission of nervous impulses.

The science of biochemistry is relatively new and Gowland Hopkins was its founder in this country. His discovery of the significance of accessory food factors, leading up to the recognition of vitamins, started the modern science of nutrition. Other landmarks were Bayliss and Starling's recognition of the part played by hormones in the blood stream, followed by Banting and Best's isolation of insulin, and Harington's synthesis of thyroxin here in Edinburgh.

Fleming working on bacterial cultures discovered the antibacterial properties of penicillin and later Florey and Chain, at Oxford, found that penicillin could be extracted in a highly purified form, and used it to treat human disease.

Modern surgery can be said to have been born in Scotland with Simpson's discovery of the use of chloroform as an anaesthetic and Lister's antiseptic technique based on Pasteur's bacteriological discoveries. A further advance of the greatest of value to surgery as a science was Macewen's aseptic technique which made surgery clean and safe, followed by his classic work on the brain and spinal cord.

If Lister was the father of modern surgery, then Manson was the father of tropical medicine, and it is particularly in this field that the British contribution has led the world. The discovery by Ross that malaria is carried by the anopheles mosquito and, much later, the work of Fairley in Australia on its prevention and cure have been of the greatest benefit to mankind. Bruce will always be remembered for his discovery of the part played by the deadly tsetse fly in the transmission of sleeping sickness and his work on Malta fever. Finlay, Adrian Stokes and Hindle stand high among the names linked with the study and prevention of yellow fever.

These were all vital efforts towards the prevention of sickness, but there is another aspect of medical practice in which the Commonwealth has taken a leading part —the promotion of health. It was Sir John Simon, the first Medical Officer appointed to a central authority, who made a careful statistical study of the causes of sickness, with a view to taking effective measures for the health of the community at large. Through his leadership health services have been provided in regular stages throughout the country. At first these were largely aimed at providing pure water, effective sanitation, and the abolition of slums ; but since the beginning of the present century the *personal* health services, especially in the case of mothers, babies, and school children, have become national in scope and lead the world.

There are two other fields in which the biological sciences play a major part. The first is in the preservation of food and in nutrition which has had the most profound economic, and social effects. The ability through freezing, drying and canning to import large quantities of food has enabled a rapidly increasing population to maintain and increase its standards of living, which would have been impossible had it been dependent on British agriculture alone. The scientific study of nutrition has made it possible to improve the health of the population and in war to feed the people with the minimum of waste.

Mort had the first freezing works in the world at Sydney, and was a pioneer in refrigeration, but success in transporting meat to Britain had to wait for the development of more reliable refrigerating plant. Since 1918 the Food Investigation Laboratories of the Department of Scientific and Industrial Research, of which Sir William Hardy was the first director, have established the basic biological knowledge on which the storage, and transport of meat, fish and fruit are now largely based.

The second field is in agriculture, where in order to compete with cheap foreign foods the most successful farmer is one who enlists the full assistance of science. Lawes, who discovered how to make and use superphosphate, and started the great fertiliser industry, was quick to realise this. He founded Rothamsted, now the oldest agricultural research station in the world, and there he and Gilbert carried out the first scientifically controlled field experiments which laid the foundation of agricultural science. Later, Biffen's pioneer work in plant breeding at Cambridge became one of the greatest contributions to the problem of feeding the world's growing population. He showed how it was possible to breed strains of wheat combining resistance to disease with high yields and good milling properties. In the field of animal breeding, the foundation of the most important aspect of British agriculture to-day, I will mention amongst the many investigators only Cossar Ewart and Crew who did so much to advance its scientific study here in Edinburgh. The mechanisation which was to revolutionise farming in all parts of the world was also under way and Britain was playing a leading part. The reaping machine, for instance, was invented by Patrick Bell in 1826 although it was not manufactured until 1853.

There is no need to point out the effect

which all these improvements, discoveries and inventions have had on Society. It is this group of biological sciences which have had the most far-reaching social results, and it is particularly during and since the last war that it has been possible to exploit them.

There is one science which I have not yet mentioned. It is both the youngest science and the oldest problem. The study of man's mind was the province of the philosopher until the middle of the nineteenth century, when it separated from him and began its independent existence as the science of psychology. The foundations were not laid in this country, but important contributions were made, both from the biological and the philosophical sides by men like Ferrier, Bain and Ward. Sully's work on child psychology was the first of its kind. But probably the most outstanding figure in this country was Galton, whose teaching is widely respected in all psychological laboratories, and who was the first to develop an interest in the mental differences between individuals—a field in which British psychology has made some of its greatest contributions. Again it is only recently that full practical advantage is being taken of the progress made in this branch of science, but the results of that application may be as important as the many more easily understood developments in the purely physical world.

THE IMPLICATIONS

The story of the British contribution to science in the past century is indeed impressive and I am very pleased to have this opportunity to pay tribute to the men whose achievements I have been discussing. But this story would not be complete without studying the wider implications of their work and examining some of the lessons to be learnt from it.

The concrete measurement and indirect effect of all scientific effort is the general improvement in the condition in which people live and work, it is in the improvement in health, in the expectation of life and standards of living. The latter, including not only food and clothing, but housing, home comforts, medical care, education, books and newspapers, recreations and travel facilities. In every one of these directions the progress that has been made has amounted to a revolution.

Not all this springs directly from science and invention. Much has been due to the politicians and administrators, and behind them to religion, morals, education, art and the complex influences which we call culture. But even there science has stood beside the authors of progress to advise, to help and sometimes to guide.

Now as science and technology are so vital to the future strength and prosperity of the British Commonwealth, the great problem is to discover the conditions under which they are most likely to flourish. The records show that both depend very much on co-operation, and upon the linking up of a long chain of discoveries, one with another; so that it is quite exceptional for the credit of a great advance to belong to one man or even to one country, although it will always require the flash of inspiration to weld the links into the chain. To-day the development of teamwork in laboratories has made this truer than ever. For many reasons, but principally because of the increasing complexity of research and its cost, such teamwork is becoming more and more the rule. We need not repine at this but it would be a disaster if the individual inquirer working in his own laboratory were discouraged out of existence.

While the quality of scientific work is determined by the quality of the scientist, the quantity of scientific output is determined by the money available. The rapid progress of science in Britain has owed much to the growing support and sympathy of government and individual benefactors and to the endowment of research by industrial corporations. However, the basic discoveries that mark the great advances depend on the accident of individual genius and are not at our command.

The scope and intensity of the progress of applied science and technology on the other hand bear a close relationship to the circumstances of the time. Technology, as the combination of scientific knowledge with the practical ability of the inventor to apply that knowledge to the solution of particular problems, comes into play with any new discovery of

scientific fact. The latest particle of truth is then developed, according to the circumstances of the time for military, commercial or medico-social purposes. It is a sad reflection that the urgent demands of modern war can produce advances that might otherwise take many years to develop, especially in the costly and uncertain experimental stages.

The rivalry between large commercial undertakings, using science to improve their products or processes as a direct means of competition, has produced a steady flow of improvements and developments. However, the fruits of this form of scientific work are sometimes open to considerable misuse. The discoveries of these commercial laboratories may be kept secret and in some cases a number of teams may be working on the same problem, which may have already been solved elsewhere. The buying up and suppression of patents and discoveries to protect equipment from becoming obsolete has also been known to happen. I am glad to see, however, a change of outlook in the growing quantity of publication of the results of industrial research.

It would seem that science has become so well established that nothing can stand in the way of its natural growth. This is far from the truth. Since the earliest times the natural conservatism of laymen has acted as a powerful brake to the adoption of new ideas, which do not rigidly conform to his notion of the correct order of things. In its most violent form it will produce unreasoning anger, utter disbelief in face of the clearest evidence or provoke plain ordinary laughter. The storm raised by Darwin's 'Origin of Species' is an excellent example where even scientists failed to keep an open mind.

The position seems better to-day and I am sure that Sir Harold Hartley, our immediate past President, spoke for all scientists when he said :—

'To-day, with our greater understanding, there is humility in the minds of all scientists. The further we penetrate into Nature's secrets the more clearly we see the ever-receding frontiers of knowledge.'

The resistance towards anything new or unexpected is balanced on the other hand by bursts of enthusiasm that some particular discovery or invention will see the end of all our troubles. The belief in the philosopher's stone seems to be just as great as ever.

As the front of pure science has advanced so its lines of communication to practical exploitation have got longer and longer. The time was when the whole process of discovery, application and exploitation could be achieved by one man. In our time a great army of scientists, technicians, inventors, designers and production engineers are required to keep the lines of communication open. Quite how important some of the members of this follow-up team have become is not always appreciated. In his presidential address in 1948 Sir Henry Tizard emphasised this point when he said :—

'All depends on good design and production. Our weakness in the war was not to be found in what was best to do, nor in the scientific work of how to do it. It was when the stage of design and production was reached that we fell short of the best standards.'

This was true already when Whitworth invented the screw micrometer, which was subsequently put into production in Germany and the United States and up to the 1914 war all micrometers had to be imported into this country.

To Professor Kipping of Nottingham goes the credit for the basic work which led to the development of silicones in Russia and the United States and yet until this year we have been dependent on imports from America of marketable silicone products.

There are many cases in the Navy where a piece of apparatus has been used operationally exactly as the inventor put it together, with all the resulting disadvantages in maintenance and efficient operation. The limitation in performance, except in some cases, is practical as opposed to scientific. Where the basic scientific principles are known by all nations the advantage lies in the good design of equipment for practical use.

A more general and far-reaching matter for concern and possibly the most vital factor affecting the industrial application of scientific research is the lack of a coordinated system of scientific and technological education in this country. Excellent as they are, the existing institutions,

which have grown up to meet particular circumstances, do not produce anything like enough trained technologists to meet the urgent needs of scientific development in industry and to provide leaders for the future. It is to be hoped that the new and rather uncertain science of education will develop sufficiently quickly to point the way to a speedy solution of this problem.

The shortage in Britain of ' personnel trained and eager to apply scientific knowledge and scientific methods to practical ends '—as Sir Ewart Smith said last year—is only one of the many shortages which the world is now facing. Among them are food, non-ferrous metals, steel alloy metals and sulphur. These very shortages are due to the scientific complexity of present-day life and it is only by science that they can be overcome. Naturally there are many ways of tackling this problem; but the most obvious are firstly by improved design to secure economy in production and the minimum use of scarce materials. Secondly by the development of substitutes made from raw materials which are still abundant. Thirdly by the reclamation of scrap and improved methods of using low-grade ores. Finally the development of renewable raw materials such as timber to satisfy the world demand for cellulose. Some of these shortages are partly due to the huge inevitable waste of war and its consequences, and partly to the lack of any comprehensive survey of the world's resources and requirements. It is only by an accurate knowledge of the world's resources that we can foresee the scope and magnitude of the future problems that science and technology have to meet and that only they can solve.

It is, therefore, good news that the Economic and Social Council of the United Nations has resolved ' to promote the systematic survey and inventory ' of those resources which are not already covered by the Food and Agriculture Organisation.

We have evolved a civilisation based on the material benefits which science and technology can provide. The present shortages are a timely reminder of the slender material foundation on which our civilisation rests and of our dependence upon science and technology.

THE CONCLUSION

The pursuit of truth in itself cannot produce anything evil. It is in the later stage when the facts dug up enter the process of application that the choice between the beneficent and destructive development has to be made. It is quite certain that it is an exception if any particular discovery cannot be used equally well for good and evil purposes. Happily the beneficent exploitation of scientific knowledge has kept pace with its destructive application.

In a mid-century article *The Times* put it this way: ' . . . It has been an age of great achievement. The lines of progress in which the Victorians trusted have been pursued farther and faster than they foresaw. Scientific discovery, from which above all their doctrines of progress derived, has swept forward on an enormous front. The conquest of the air has made possible an intercourse and understanding between distant peoples such as our ancestors could not imagine—and it has been diverted to the vast destruction of men and cities. The invention of wireless telephony has opened a channel through which liberating truths might be proclaimed to all the listening earth—and every would-be despot has used it to suborn the blind masses into the worship of false gods. The medical art has performed miracles; the cures of immemorial pestilences have been found, infancy has been safeguarded and old age tended, so that the normal expectation of life has been extended by years—aside from the new and universal apprehension of sudden death.'

To my mind it is vital that the two sides of scientific development are fully and clearly understood, not only by the research scientist, inventor, designer and the whole scientific team, but also by all laymen. The instrument of scientific knowledge in our hands is growing more powerful every day, indeed it has reached a point when we can either set the world free from drudgery, fear, hunger and pestilence or obliterate life itself.

Progress in almost every form of human activity depends upon the continued efforts of scientists. The nation's wealth and prosperity are governed by the rapid application of science to its industries and

commerce. The nation's workers depend upon science for the maintenance and improvement in their standard of health housing and food. Finally superiority or even our ability to survive in war is a direct measure of the excellence and capacity of the scientific team.

This team of research workers and engineers has a dual responsibility, one for its work and the other as informed citizens, and it can only fulfil its proper functions if its members have a sound general education as well as a thorough training in science. It is no less important that the people who control the scientific machine, both laymen and scientists, should have a proper understanding and appreciation of what science has grown into and its place among the great forces of the world.

Ladies and Gentlemen, it is clearly our duty as citizens to see that science is used for the benefit of mankind. For, of what use is science if man does not survive?

A HUNDRED YEARS OF METEOROLOGY (1851–1951)

Address by

SIR DAVID BRUNT, SEC.R.S.

PRESIDENT OF SECTION A

THE EARLIEST METEOROLOGICAL HISTORY

BEFORE we consider the development of meteorology during the last hundred years, we shall find it profitable to devote a few minutes to outline the earlier development of the subject. The earliest book on the subject of meteorology of which we have any knowledge is the *Meteorologia* of Aristotle, written in the fourth century B.C. There are also extant small treatises on *Winds* and on *Weather Signs* by Aristotle's pupil Theophrastus, and many of the Weather Signs of Theophrastus are to be found in the weather lore of widely separated countries, in almost identical words.

Aristotle was by no means the first Greek author to write of the weather. Thus Pindar wrote at the dawn of the fifth century B.C.—'The wise know the wind that shall blow on the day after to-morrow, and are not wrecked through eagerness for gain.' In so writing Pindar set up a standard of weather forecasting which even to-day is difficult of attainment, except on rare occasions.

The physical concepts of Aristotle's day were so inadequate that his writings, while often shrewd and clear, give only a limited insight into the processes of weather. Nevertheless, Aristotle and Theophrastus dominated the meteorological thought of Europe for nearly 2,000 years, and during this long period writings on meteorology were almost entirely restricted to translations of, and commentaries on, the writings of Aristotle and Theophrastus, and the accumulation of 'Weather Lore.'

It was in the second half of the seventeenth century that really independent thought was first given to the subject of meteorology. This was a period of scientific awakening, and the foundation of the Royal Society in 1660 provided a powerful stimulus to the discussion of scientific problems. The birth of scientific meteorology came with the publication in 1688 of Halley's paper on the Trade Winds, and of Hooke's many brilliant suggestions in *Posthumous Works* (1705). But the writings of Halley and Hooke failed to produce any deep and lasting interest in the dynamics of the atmosphere, although from their day onward into late in the nineteenth century physicists took some interest in the physics of the atmosphere. It is of interest to note that the typhoon or tropical cyclone was described by Dampier in 1697. But it was in the early years of the nineteenth century that real progress began.

At the end of the eighteenth century Humboldt plotted charts of the distribution of pressure and temperature over the globe, and so initiated what was later known as the 'synoptic' approach to meteorology. Already in 1735 Hadley had improved Halley's theory of the Trade Winds, by taking account of the rotation of the earth, showing that it produced a deviation of air to the right in the northern hemisphere, and to the left in the southern hemisphere. Humboldt showed from mountain observations that temperature decreased with height at a rate of about 1° C. per 500 ft. Dalton described the laws of vapour pressure in 1801–03, and showed that clouds are formed by the expansion and cooling of ascending damp air. In 1805 Laplace showed that the rate of variation of pressure with height is equal to the weight of unit volume of air. In 1820 Brandes published a discussion of the weather of each day of the year 1783, and followed the development of individual

depressions, showing that around a centre of low pressure the winds blew in the counterclockwise sense around the centre, and across the isobars from high to low pressure. The work of Brandes was carried on by Dove, Espy and Loomis, by whom the central region of the depression was regarded as a region of ascending air. It was only in 1863 that Galton brought the centre of high pressure into the general scheme, regarding it as a region of descending air ; and it was he who then gave to the high pressure region the name of ' anticyclone.'

The observations used by the early investigators were made either by private individuals or by scientific institutions, and as the exchange of data by letter was a slow process, a map of the weather conditions over a wide area could only be made long in arrear. Nevertheless, by the middle of the nineteenth century, a fairly clear conception of the conditions in depressions had been achieved.

In the 1840's, the invention of the electric telegraph, and its fairly wide installation, provided a quicker means of interchange of meteorological observations. It was soon realised that weather depended, not on the height of the barometer, but on its rate of variation in the horizontal, and the use of the barometer as a ' weather glass,' which had persisted for nearly 200 years, was thus discredited.

The State of Meteorological Knowledge in 1851

In brief, we may say that at the beginning of 1851 the general distributions of pressure, temperature and wind over the globe were known at least in outline ; the distribution of weather in depressions was known from the study of individual depressions ; the rate of fall-off of temperature with height in the lower layers was appreciated ; the mode of formation of clouds was understood, though there would appear to have been differences of opinion concerning the formation of rain. The classification of clouds, in much the same form as we know it to-day, was proposed by Luke Howard in 1803. Ideas concerning radiation and absorption were somewhat nebulous, and were first clarified by Tyndall in the 1860's.

The ' synoptic ' chart was not yet born, and weather forecasting had not been attempted seriously. This may seem strange in view of the fact that about 1780, after discussions with Laplace, d'Assy, Vandermonde, de Montigny, and others, Lavoisier had written a note in which he defined with amazing precision the organisation of weather forecasting much as we know it to-day. The electric telegraph had been used in U.S.A. since 1849, to collect observations of weather, and in England it had been used by Glaisher since June 1849 to collect weather reports for publication in the *Daily News*. Lithographed copies of weather charts based on these reports were sold to the public at the Exhibition of 1851, at the price of one penny each.

Ocean Winds and Currents

Lieutenant (afterwards Admiral) M. F. Maury of the U.S. Navy, who in 1839 had met with an accident which incapacitated him for service afloat, set to work to collect observations of ocean currents and winds, with a view to producing world charts of the main systems of air and ocean currents. He was able to publish such charts in 1848, and from the charts he was able to recommend that sailing ships proceeding from England to Australia should cross the Equator much farther west than had been customary. The result was to reduce the average length of the outward passage from 124 days to 93 days ; this passage was actually completed in 63 days (Liverpool to Melbourne) by the ship *Lightning* in 1853, while the return passage was completed in 60 days by *Thermopylae* in 1878–79, and again in 1879–80. This reduction of one month in the time taken for what must have been an incredibly dreary journey was perhaps the most dramatic achievement of meteorology in all its history, and apart from its economic importance, it made meteorologists aware of the value of international co-operation.

The Founding of the Royal Meteorological Society

At the beginning of the hundred years we have to consider specially, there was considerable enthusiasm for meteorological investigations, and notably for the systematic collection of observations. This

enthusiasm had led to the foundation of the British Meteorological Society in April, 1850. The Society had two aims, first, to collect, and publish summaries of, observations made by its members, and secondly, to discuss papers submitted by its members. The first of these aims was prosecuted most vigorously under the driving force of the Secretary, James Glaisher, who organised a system of well-distributed climatological stations throughout England and Wales. Monthly summaries were published in the Annual Reports to the Council up to 1871, then in the *Quarterly Journal* up to 1880, and in a separate publication known as the *Meteorological Record* from 1881 up to and including 1911. Since 1912 such summaries have been published by the Meteorological Office, in the Monthly Weather Report. Thus for 60 years the Meteorological Society acted as a public memory of the weather, and took upon itself the duty of devising suitable instruments, and collecting a large mass of standard observations.

The Scottish Meteorological Society, founded in 1855, performed similar service for Scotland, until in 1912 it was incorporated in the Royal Meteorological Society.

The Role of the British Association

In 1842 the British Association acquired Kew Observatory, which had ceased to be an astronomical observatory in the preceding year, with the view of using the building for the prosecution of investigations into various branches of geophysics, and as a depository for books or other property of the Association. Professor Wheatstone, who superintended the work of the officer in charge of the Observatory, established during the first year of operation a set of standard meteorological instruments, a set of self-recording instruments, and apparatus for recording the electrical state of the atmosphere. The Observatory soon became a recognised testing-house for instruments, particularly thermometers, barometers, hydrometers and magnetic instruments. The Kew Committee of the British Association, with J. P. Gassiot as its Chairman, exercised a general supervision over the work of the Observatory, and laid down the main lines of activity. In 1852 the investigation of the upper atmosphere made its first appearance on the programme of the Kew Committee, and the balloon ascents by Welsh, Superintendent of Kew Observatory, were made in that year, later to be continued by Glaisher, Secretary of the Meteorological Society. The special contribution of the Kew Committee to meteorology was the introduction of recording instruments for pressure and temperature. The connection between Kew Observatory and the British Association lasted from 1842 to 1872, when the Observatory passed into the control of the Royal Society. During this period of 30 years, the Association disbursed a total of £12,300 on the work of the Observatory.

Kew Observatory became the central observatory for the series started later in connection with the Meteorological Office, at Falmouth, Stonyhurst, Glasgow, Aberdeen, Armagh and Valencia, all of which were equipped with self-recording instruments similar to those at Kew, the close relations which had for some time lasted between the Meteorological Office and Kew Observatory being maintained. In supporting Kew Observatory the British Association rendered a great service to the geophysical sciences.

The British Association has always maintained a keen interest in the upper air. One of the most valuable early publications on the subject was the report on the then state of our knowledge compiled by E. Gold and W. A. Harwood, issued by the Association in 1909. The Association accorded its support to the meteorological Observatory on Ben Nevis. Also for a number of years, the data given in *British Rainfall* were more fully discussed in reports printed by the Association. In meteorological matters generally, the British Association has supported with funds for purchase of equipment, and with facilities for publication, many efforts at developing hitherto unexplored aspects of the subject.

The Establishment of National Meteorological Services

Maury was not satisfied with the mass of observations of ocean winds and currents which he had plotted on the 1848 charts,

and he moved the U.S. Government to invite all the maritime nations to send representatives to a conference held in Brussels in 1853. The conference laid down a plan of observations to be made at sea, a plan which was immediately put into operation by all the maritime nations of the world. It also recommended the establishment of national meteorological services in all countries.

Early in 1855, in accordance with this recommendation there was established the Meteorological Department of the Board of Trade, later to become what we now know as the Meteorological Office, Air Ministry. Its Superintendent was Admiral Fitzroy, who had commanded the *Beagle* on Charles Darwin's voyage round the world. At its initiation its stated aim was to collect observations from British ships for the benefit of the Navy and the Mercantile Marine.

During the summer of 1860 arrangements were made for the regular daily communication to the office in London of reports of the state of the weather at 15 land stations in the United Kingdom, for receiving daily weather telegrams from various stations in Europe via Paris, and for communicating to Paris observations of the state of the weather in the British Isles. Thus the functions of the Meteorological Department were not only extended to include the land as well as the sea, but were at the same time given an international character.

The establishment of the Meteorological Department of the Board of Trade was quickly followed by the establishment of Central Meteorological Offices in other countries, and by 1860 there was a sufficient *réseau* of observing stations in Europe to make the international exchange of information via Paris of real value to the co-operating services.

In February 1861, Fitzroy began the issue of storm warnings, and in August of the same year began the issue of daily forecasts of weather to the newspapers. The use of weather maps for such purposes was regarded in scientific circles with suspicion, and was described as 'empirical,' a word which, in the mouths of scientific men, is a heavy missile. Fitzroy's death in April 1865 furnished an occasion for consultation between the Government and the Royal Society, leading inevitably to the setting up of a committee to 'consider certain questions relating to the work of the Meteorological Department.' As a result of the deliberations of the Committee the Meteorological Department was removed from the Board of Trade, and became the Meteorological Office, which, until 1920, was administered by a Royal Society Committee.

The Meteorological Office

It is convenient at this point to summarise the various stages in the history of the official meteorological service in this country :

1. Meteorological Department of the Board of Trade (1855–67).
2. Meteorological Office, administered by a Meteorological Committee, nominated by the Royal Society (1867–77).
3. Meteorological Office, administered by a Meteorological Council, consisting of the Hydrographer of the Navy and five (later six) paid members (1877–1905).
4. Meteorological Office administered by a Director (Sir Napier Shaw), with an unpaid Advisory Committee, formed of two representatives of the Royal Society and one each from the Treasury, the Admiralty, the Board of Trade, and the Board of Agriculture (1905–20).
5. Meteorological Office, Air Ministry, administered by a Director responsible to the Secretary of State for Air, with the Advisory Committee increased both in numbers and in departmental representation, and taking a less active part in the details of the work of the Office (1920–present-day).

The transition from one régime to the succeeding one was in general brought about by some kind of general enquiry into the efficiency of the service. The Committee set up after the death of Fitzroy in 1865 recommended the continuation of the observations at sea, the discontinuance of the daily forecasts, the improvement of the storm warnings, and above all 'that steps should be taken for establishing a full, constant and accurate system of showing changes of weather in the British Isles.' With this last purpose in view, they recommended (*a*) the establishment of six self-recordings observatories ; (*b*) the

daily collection of observations by telegraph, and (*c*) the issue of a daily weather report.

The report was accepted, and a sum of £10,000 was provided in the estimates for 1867, as a grant-in-aid, the Royal Society being asked to nominate a Committee to control the administration of the grant. The Meteorological Office then ceased to be a department of the Board of Trade, and henceforth, until 1920, when it became a department of the Air Ministry, it was administered by a Meteorological Committee or Council, in which the Royal Society had considerable influence. During the whole period 1867–1920 the Office was financed by a Treasury Grant-in-Aid.

For some years the observations were published by the reproduction of facsimile reprints of all the autographic charts, as well as of such curves as were obtained by calculation, in the *Quarterly Weather Report*. The very first report gave, as an illustration of what could be achieved by means of self-recording instruments, photographs of charts of pressure and temperature records, exhibiting what we should now call a ' line-squall ' (on March 8, 1867). The Report states :—' Sudden and peculiar changes of weather of this kind have been remarked by Mr. Airy and others as of frequent occurrence with sudden changes of wind.' Sir Napier Shaw (in 1934) made a dry comment on this : ' If they had kept " right on to the end of the road " at that time, the combination of maps and records might have set out the doctrine of fronts in 1869 instead of 1919.'

In December 1866, immediately before the appointment of the Meteorological Committee, the issue of both Storm Warnings and Forecasts had been discontinued on the ground that the state of meteorological science was not sufficiently advanced to enable the predictions to be based on a sound basis of principle. In response to popular demand, the storm warnings were fully restored by 1876, and daily forecasts were recommended in 1879. With the passage of the years, there was accumulated in the Meteorological Society and in the Meteorological Office a large store of standard observations made at a large number of stations. From time to time new instruments were devised, so that fuller or more accurate observations could be made. But until the early years of the twentieth century it is not an unfair criticism to say that in general the only treatment to which the observations were submitted was a statistical or arithmetical treatment. Diurnal and annual variations were computed, while in some cases a Fourier analysis was made of selected observations, but the growth of a physical understanding of atmospheric processes was slow.

Before we go on to later events, it is not without interest to consider in a broad way what had happened in the Meteorological Office during the years 1855 to 1905. The very live interest in weather and weather charts which led to the birth of the Meteorological Society in 1850, and of the Meteorological Department in 1855, lasted, perhaps intermittently, some three decades, and by May 1897, when Shaw became a member of the Meteorological Council, the enthusiasm had entirely gone from the office. Meteorology was then, indeed, in the Slough of Despond. Such scientific knowledge as could be found in the Office was restricted to the members of the Council, there being not a single member of the Office staff who had received a scientific training. When Shaw joined the Meteorological Council the other members were, Sir Richard Strachey (80), a great administrator and an enthusiast for the computation of accumulated temperatures and of harmonic coefficients ; Sir Francis Galton (75), who had been the chief creator of the Office as a scientific establishment ; Alexander Buchan (68), Secretary of the Scottish Meteorological Society ; Professor G. H. (later Sir George) Darwin ; and Admiral Sir W. Wharton, who filled the office of Hydrographer for 20 years.

The Council took no interest in forecasting, nor indeed did any of the permanent staff of the Office. It is clear that the organisation of the Office, with scientific knowledge to be found only on the Olympian heights of the Council, was not in practice an efficient one. Under this régime the Office passed through its dullest stage of growth. By this time, too, physicists in general had ceased to take an active interest in the problems of the atmosphere. Like the priest and the

Levite, they ' passed by on the other side.' The lack of scientific training among the men who devoted their time to the consideration of daily observations made impossible any serious advance in the understanding of the processes of weather.

It was to such an Office that Shaw first came as a member of the Council in 1897, to become Secretary of the Council in 1900, and Director of the Office in 1905. Shaw's services to meteorology should be judged against the background of the Meteorological Office to which he came. He contrived to instil some enthusiasm into the Office, and in the end lifted it from the Slough of Despond. After his appointment as Director he succeeded in attracting a few scientific graduates into the Office, and by 1910 he had four such men on the staff. It is no great exaggeration to say that Shaw found meteorology an exercise in arithmetic, and left it a branch of physics, the contributions which he and his early scientific colleagues in the Office made to the subject being of prime importance.

Shaw's contributions to meteorology were more far-reaching than his writings. He brought to all questions an open mind, and a readiness to take decisions when convinced. One instance of this will be cited.

The destruction of the Tay Bridge by an exceptionally strong gust in a gale on December 28, 1879, awoke an interest in the strength of winds in the gusts. This information could not be obtained by the cup anemometer then used as a standard instrument. A joint Committee to investigate this problem was set up by the Royal Meteorological Society and the Meteorological Office, and W. H. Dines, then aged 24, was appointed to do the experimental work. One of the principal results of the investigation was the invention by Dines of the Pressure Tube Anemometer. One of these instruments was set up in 1895 at Holyhead, which had been selected as the location for the investigation, on account of the exceptionally good exposure which was available there. In the same year pressure tube anemometers were set up at Kew and at St. Mary's, Scilly, but there was considerable delay in the general acceptance of the Dines Anemometer as a standard instrument, a delay which reflects little credit on the reigning pundits of meteorology of those days. The papers relating to the Dines Anemometer came into Shaw's hands soon after his appointment to the Meteorological Council, and he took prompt action to have a number of the anemometers constructed and installed at observing stations.

During the war of 1914–18, the staff of the Meteorological Office was greatly increased to meet the ever widening demands for meteorological information. A Meteorological Section of the Royal Engineers was formed, to meet the needs of the Army and of aviation in the field. Separate Meteorological Services were formed later for the Royal Air Force and the Navy. After the war, in 1920, these services were incorporated in the Meteorological Office, which then became a Department of the Air Ministry. At the same time, the *British Rainfall Organisation*, which had been founded by G. J. Symons in the early 1860's, was incorporated in the Meteorological Office. This organisation collected rainfall data from a very widely spread network of observers, which was of substantial value in relation to water supply. Its work was one of the most successful practical ventures in meteorology, and it still continues its activity as a branch of the Meteorological Office.

There has been no substantial change in the structure of the Meteorological Office since 1920, apart from the removal of Naval Meteorology back to the Admiralty, though the wider field of human activity for which the Office now supplies information has demanded an enormously increased staff. The outstanding change within the Office since 1920 has been the recognition of the need for the provision of staff to undertake research in the many problems which arise in connection with new and old fields of application of meteorological knowledge.

International Co-operation

In 1872 an International Conference was held in Leipzig, to discuss the extent of the observational material collected by the various national organisations. At this Conference a permanent international committee was set up, with Buys Ballot as

Secretary, and in 1873 at an International Meteorological Congress, held in Vienna, arrangements for international co-operation were put upon a sound basis. The co-operation then initiated has only broken down during the two world wars, but has been re-established for some years from the breakdown in the war of 1939–1945. From time to time there have been changes in codes and in the amount of information given in the coded messages, but the general goodwill underlying the exchange of information has sufficed to maintain the continuous collaboration of the meteorologists of the world, in what is now known as the World Meteorological Organisation.

The Upper Air

The exploration of the upper air, which began with the balloon ascents of Welsh and Glaisher, remained effectively in abeyance until about 1893, when box-kites were first used to raise registering instruments for temperature and humidity into the free atmosphere. Free balloons, or sounding balloons, were first used in the same year. A considerable volume of such observations was accumulated by W. H. Dines and C. J. P. Cave, and Cave also made regular observations of upper winds using small free rubber balloons (pilot balloons), a method which became a standard form of observation of upper winds for the purposes of the Royal Air Force and of artillery during the war of 1914–18.

The use of sounding balloons led, round about 1900, to the discovery of the stratosphere, as a region in which the steady fall of temperature with height observed lower down is replaced by temperature remaining uniform, or showing a small increase or decrease with height. The doubts which were voiced as to the reality of this change of temperature régime were finally disposed of in 1909, when E. Gold showed that the existence of the stratosphere was a direct consequence of radiation and absorption by water vapour and carbon dioxide in the upper layers of the atmosphere.

The fall of temperature in ascending moist air was first fully explained by Sohncke in 1875, and Hertz produced in 1884 a diagram on which such changes could be plotted. The subject was further discussed in 1900 by Neuhoff, who produced an improved variant of the diagram of Hertz, while von Bezold devoted a number of papers in 1884–1906 to the thermodynamics of moist air. It was thus made possible to plot the observations of temperature and humidity in the upper air on a diagram which could readily be interpreted. Sir Napier Shaw produced in 1925 an Entropy-Temperature diagram (the Tephigram) which served the same purpose and has come into general use in many countries. On such diagrams observations of temperature and humidity in the upper air could be plotted in a manner which made it possible to interpret them in terms of stability or instability, levels of condensation, and of levels of base and tops of clouds. Such diagrams were therefore a valuable addition to the equipment of the practical forecaster, provided that reliable observations were available.

The Depression and Weather Forecasting

The forecasts of the first 50 years of the Meteorological Office were largely based on the assumption that weather travels in a general west-east direction, and that any depression will continue to move along the path it has followed during the past 6 or 12 hours. The distribution of weather within a single depression had been laid down by Abercromby in 1885, and the early forecasters tended to assume that this distribution would also travel eastward without serious change. There was little or no idea of continued development within the depression, and perhaps the first important step in establishing the physics of the observed weather came with the publication of a memoir by Shaw and Lempfert in 1906, entitled *The Life History of Surface Air Currents* (published H.M. Stationery Office). The publication contained so many maps that, for economy's sake, only 350 copies were printed, with the result that, for many years, it has been extremely difficult to obtain. In the introduction to this publication Shaw gave a simple diagram to illustrate the air currents in a depression, giving a close approximation to what is known as the Norwegian or Polar Front theory of the nature of the depression. I would regard this memoir as Shaw's greatest written

contribution to meteorology. It could have made meteorological history had it only been followed up. I have been told that soon after its publication Shaw spent a week-end with W. H. Dines, who had just then evaluated the correlations between pressure at 9 km. and the temperatures from that level to the ground, and had deduced from these the idea that the origin of the depressions should be sought at the 9 km. level. Shaw was so impressed by the ideas put forward by Dines that he took no further interest in the consequences of the *Life History*, and so missed the opportunity of finding the life history of the depressions. We might here quote Shaw against himself—' had he kept right on to the end of the road ' he might have set up the doctrine of fronts in 1907. We owe the analysis of the life history of the depression to J. Bjerknes and H. Solberg in a paper entitled *Life Cycle of Cyclones and the Polar Front Theory of Atmospheric Circulation*, published in 1922.

In the majority of cases the polar front method of forecasting does not contradict violently the deductions made by other methods, but the newer method gives a clearer understanding of the genesis of the weather.

Shaw and Lempfert had shown, in the memoir of 1906, that the rainfall within the depression was largely localised in two zones, and the Polar Front Theory was a restatement of this, with the added, and remarkable, contribution that the depression had a life history. The general idea that the depression is a region of clash between two air masses, each of which is largely homogeneous, led to a much clearer understanding of the physics of the weather in the depressions.

No detailed account of the Polar Front theory will be attempted here, as it is impossible in a brief account to avoid oversimplification, which leaves the reader with the feeling that weather forecasting is simple, whereas it is in fact an extremely complicated problem. Improvement in forecasting is to be looked for in the treatment of the atmosphere as a three-dimensional medium, and finding methods of representing the distribution of temperature, etc., in three dimensions, on a system of two-dimensional charts.

The recent development in the use of synoptic charts has been concentrated on the representation of upper air observations. Since any standard set of observations of upper air conditions gives temperature and humidity at specified pressures, it is possible to evaluate the pressure at any particular height, and to plot for that height a synoptic pressure chart covering a wide area. But in practice it is found preferable to draw on a chart the contour lines showing the height above sea level at which the pressure has fallen to some particular value, say 700 millibars, or 500 millibars. Another type of chart which has been found to be of practical value is one showing what are called thickness lines, and which represent the thickness or depth of the column having, say, a pressure of 1,000 mb at its base, and of 700 mb at the top. This chart shows immediately the distribution of warm and cold air masses and their relation to the distribution of the broad currents and weather systems in the atmosphere.

The Effects of the World Wars I and II on Meteorology

The war of 1914–18, with its rapid development of aircraft, and their wide use for control of artillery fire, for general observation of enemy movements, and for bombing, as well as the use on a large scale of artillery for bombarding unseen targets, led to the need for regular observations of wind, both to control the course followed by aircraft, and to make possible the accurate correction of artillery fire for the effects of wind. At the same time the need to correct artillery fire for the effects of density led to the installation of wet and dry bulb thermometers on the struts of aircraft, in such positions that they could be read by the pilot or his observer at various stages of ascent to the highest level attained by the aircraft. By the end of the 1914–18 war the use of pilot balloons for observing upper winds, and of aircraft for measuring temperature and humidity in the free air had become established as standard methods, and the pilot balloon is still in use for its original purpose at aerodromes and other meteorological stations.

The use of gas during the 1914–18 war, which made it appear at least possible that the same method might be

used in a later war, led to a concentrated attack on the problems of diffusion of gas emitted in any given meteorological conditions. The effective agency in such diffusion is turbulence in the air. The sinking of S.S. *Titanic* early in 1912 led to an investigation of ice-conditions and the formation of fog in the north Atlantic, and the meteorological part of the investigation was entrusted to G. I. Taylor. As a result of these investigations Taylor published in 1915 and 1922 two separate mathematical modes of attack on the problems of turbulence, and later developments of one of these methods by O. G. Sutton led to considerable advances in our understanding of the effects of turbulence, and a formal statement of the concentration in a cloud of gas in terms of the rate of emission, height above ground, distance from the source of emission, and the degree of atmospheric stability as represented by a parameter defining the rate of variation of wind with height. The advance thus made in our knowledge of turbulence was made possible by the interest of the fighting services in the investigation, which ensured the provision of equipment, facilities for experiments, and staff.

During the war of 1939–45 the greatest stimulus to instrumental meteorology came from the invention of radar, and the use of short-wave radio generally. Long before the war efforts had been made in many meteorological services to develop radio apparatus which could be carried up into the upper atmosphere by free balloons, and which would at fixed intervals send out radio signals from which could be deduced the pressure, temperature, and humidity of the air at the height of the balloon. During the war an increased effort to improve the efficiency of such apparatus led to the development and manufacture of reliable instruments, which are still used for the collection of upper air information. Some use has also been made of rockets such as the V_2, to make measurements of temperature up to great heights above the ground. A method of measuring the wind in the free air by using radiolocation methods to fix the position in the atmosphere of free balloons at a succession of instants was developed early in the war. Both radio methods are now in use as standard methods of obtaining measurements of conditions in the free air. They have the great advantage that they can be used in cloudy weather, in rain or fog and even in high winds up to considerable heights.

The practical demand for upper air temperatures and humidity in connection with the needs of artillery during the 1914–18 war led to the wider use of aircraft for making such observations. By the end of the war meteorologists looked forward to a great improvement in weather forecasts as a result of the availability of such upper air information. The improvement achieved in the inter-war years was however disappointing and it was only in the last 10 years that meteorologists learned to use such data in a manner that could throw some light on the three-dimensional structure of the atmosphere.

The Post-War Era

In the post-war era the much enlarged meteorological services in all the larger countries of the world has led to a much increased effort to solve some of the fundamental problems of meteorology. Among problems which have received much increased attention are :

(*a*) The nature of the depression of middle latitudes.
(*b*) The effects of atmospheric radiation and absorption on the heat balance of the atmosphere.
(*c*) The formation of cloud, rain, ice-crystals and snow.

A few words may appropriately be said here as to the results of these attacks.

(*a*) A very large volume of papers on the kinematics of the depression, has appeared in the last 8 years, but we are not yet in a position to predict where the next depression will form, or even to explain the precise conditions which led to the formation of yesterday's depression.
(*b*) The approximate balance of radiation and absorption in the atmosphere can now be evaluated by graphical methods evolved by Elsasser in the United States, and by Robinson in England.

(*c*) We have come near to understanding the precise conditions in which condensation in the form of ice occurs, while the problems involved in artificial stimulation of precipitation will form the subject of a later discussion at this meeting.

Soon after the first world war, the development of more efficient aircraft made it possible for aircraft to fly in weather which would previously have been regarded as unsuitable for flight, but a new peril to aircraft was quickly revealed, the deposition of ice on the aircraft. The meteorological factors which produce this deposition are still not completely understood.

With the still further advance of aircraft design, which has, within the last decade, made possible flight at heights not previously attained, observation has revealed a new phenomenon, to which the name of *jet stream* has been given. This is a very fast stream of air, occurring at levels of 15,000 feet to 40,000 feet above sea-level, moving at speeds of 130 knots or more, in a general west to east direction, and traceable for great distances downwind, but sharply restricted in the vertical and the transverse directions. On the poleward side the decrease of speed attains as much as 100 knots in 100 miles, while on the equatorial side a decrease of 100 knots is only attained in 300 miles from the central axis. In the vertical direction the decrease of horizontal speed may attain 80 knots in a range of height of 12,000 feet upward or downward from the axis of the stream.

It will be thus realised that the jet-stream is a relatively narrow, intense stream of fast-moving air, the possible occurrence of which must be taken into consideration in plans for flight in the stratosphere.

Meteorology at the British Universities

The teaching of meteorology at the Universities has shown a considerable lag in Great Britain, as compared with most European countries. Sir Arthur Schuster established a Readership in Meteorology in Cambridge in 1907, with E. Gold as the first Reader (1907–10), followed by G. I. Taylor (1910–13). The contribution to meteorology of these two Schuster Readers leaves little doubt as to the value of the Foundation which started their interest in the subject. During the years from 1905 to the beginning of the war in 1914 Sir Napier Shaw holding the rank of Reader in Meteorology in the University of London, gave each session a series of lectures on various aspects of meteorology to such students as might be interested in the subject. After his retirement from the Directorship of the Meteorological Office in 1920 he was persuaded by his friend Sir Richard Glazebrook, who was then Zaharoff Professor of Aeronautics in the City and Guilds College, to accept a part-time Chair of Meteorology to be attached to the Department of Aeronautics. He held this post until 1924, when he was succeeded by Sir Gilbert Walker, who was also part-time Professor, and whose attachment to the Imperial College was later changed from the Department of Aeronautics to that of Physics. Sir Gilbert retired in 1934, and I was then appointed as a full-time Professor, at first attached to the Department of Physics, but after a few years as independent Professor, with a separate Department. The expansion of the Department of Meteorology at Imperial College, which began in 1939, has continued since the end of the late war, and it now has facilities for advanced teaching and research in experimental, synoptic, observational and theoretical meteorology. In recent years there has been a steady growth of interest in meteorology at other Universities and there has been great activity notably at Cambridge and Oxford, but the Chair of Meteorology at Imperial College is as yet the only one in the whole of the British Commonwealth.

Another aspect of meteorological research should be mentioned. During the early years of the last war when a Meteorological Research Committee was set up by the Air Ministry, that Ministry, with the concurrence of the Treasury, agreed to place at the disposal of the Royal Society for use in promoting research into the heat balance of the upper atmosphere, a sum of £4,000 per annum for a period of five years. This programme of research is administered by the Gassiot Committee of the Royal Society, which has been

successful in obtaining the help of a number of distinguished scientists, under whom at a number of Universities there have worked teams of able young men. The projects being investigated have been rather long-term in character, and it is scarcely possible to summarise the results so far obtained. But now that the initial five-year term has expired, the grant, increased to £5,000 per annum, has been renewed for a further period of five years, and there is a prospect of thus achieving results which would have taken many years for isolated workers to reach.

A Century's Growth.

The growth of the official Meteorological Service in this country can be assessed by consideration of its cost. In 1867 the total cost was met by a Grant-in-Aid of £10,000, in 1920–21 an allocation of £120,000 was provided in the Air Ministry Estimates, while at the present time the total annual cost is close to £2,000,000. The increase in cost of the service, which indicates an enormous growth in its activity, is matched by the increase in the interest taken by the public in the weather and in weather forecasting This interest is clearly shown by the widespread agitation for the revival of the hourly broadcasts of weather information and forecasts known as AIRMET, which were terminated in March 1950. This series of broadcasts was regarded by a wide public as the most valuable service rendered by the Meteorological Office.

CHEMISTRY AT THE MID-CENTURY

Address by

SIR CYRIL HINSHELWOOD, FOR.SEC.R.S.

PRESIDENT OF SECTION B

THE impression which the historian gives to the layman, and no doubt also to the initiated, is that in Britain the year 1851 with its Great Exhibition symbolised a blossoming of hope and faith. There was a sense of a wondrous springtime in the achievements of this nation, and indeed of the World. The Exhibition itself displayed the flowers of material and intellectual discovery, and held out the promise of fruits which would endlessly increase in splendour and abundance. The mood of confidence was long sustained. It was nourished by the spate of scientific investigation, and strengthened by victories over ill-judged opposition. The exuberance of the age passed beyond the bounds of the material and even of the intellectual, and by a subtle irradiation pervaded the aesthetic and moral spheres. The eternal values themselves were felt in some way to be continually augmenting and widening their beneficent sway.

The reaction from the spirit of this most balmy time has been sad indeed. Hopes were disappointed. The hostile critics of the newer knowledge rallied, and men of science themselves have at times felt disoriented and uncertain of their aims. Some have even confessed to a feeling that their scientific functions are subordinate to economic and political ends, and others have fallen back before the complacent assertions of their detractors that science has no concern with values—as though truth itself were not among the greatest of these.

Some of the Victorians in the enthusiasm of their conquests thought perhaps that the inmost citadels of Nature would soon fall before them. Their hope was an illusion : Nature, it seemed, scorched the earth and fled before the seekers into a desert where they were left ' alone and thirsting in a land of sand and thorns.'

Recovery from the reaction of pessimism and dejection is far from complete. Yet the defeat, as so often, was really victory. The facile prize was lost, the goal receded, yet the realm of potential discovery expanded in a way transcending all that some of our predecessors in their philosophy had dreamt of.

Perhaps at this moment there may be profit in attempting to take stock of the realities of the situation, and to examine against this general background some particular themes of interest to the science of chemistry.

In what fields have the great chemical events of the past century unfolded themselves ?

First there has been the vast evolution of chemical theory and of our views about the basic substratum of things.

Next there has been the perfection of the arts of synthetic chemistry, the acquisition of the lapidary skill with which the chemist places atoms into settings of his own devising and desiring.

There has grown up also the conception of energy and the knowledge of its subtle laws, and all the understanding of the intimate mechanism of chemical reaction which becomes possible in the light of these doctrines.

At every stage there have been far resounding intellectual echoes and revolutionary practical results. And now at this mid-century the chemist after a long and eventful approach stands face to face with the problem of life itself.

As the personal history of a monarch sometimes typifies his era, so the evolution of the central theory of chemistry, the atomic and molecular theory, serves to reflect some of the profound changes in thought and intellectual attitude which have in the past few generations come over

science as a whole. I shall begin by saying something of this story.

The atomic theory in its inception explained the quantitative laws of chemical composition, but it gave no clue, in its early form, to the nature of affinity, though naturally and properly people began to ask the naïve question: why do atoms combine?

Are valency bonds material projections? At first such an idea may have seemed attractive but on closer scrutiny it rapidly assumed a somewhat nonsensical air. If an object possesses an excrescence, this can, at least in thought, be broken off, and the conception of the indivisible atomic unit is gone. There is, therefore, something of an inward contradiction in the very attempt to explain chemical union in this way. This very simple example of a logical awakening typifies in its primitive way much that has followed.

With the discovery of the electrical constitution of matter the ideas of excess or defect of electrons and of resultant electrostatic forces, of complete electron patterns and of incomplete structures striving for completion, the theory of valency took a more sophisticated turn, and on the afternoon of the struggle it seemed that the prize was won. But in the early light of morning the deeper difficulties appeared still formidable. Electrostatic attraction itself is profoundly mysterious, and indeed the whole idea of force is beset with difficulty. And even apart from this, there proved to be no simple means of determining in general whether the patterns of positive and negative charges which make up two atoms should in fact attract or repel on balance.

The solution to this question came in its turn, and from the quantum laws, though even from these laws only in their most finished and abstract form. If, to take the simplest example, we ask why two hydrogen atoms unite to form a molecule, the modern answer is that they constitute a system for which the quantum rules admit a lower energy than is permitted for the isolated pair. But this abstract prescription is admissible only if the electrons have different values of an obscure quantity called their spin—and about this little is really known save that it possesses one or other of two possible values.

The code, however, which defines these requirements about the spin implies properties of electrons quite unlike those of any particles known to gross macroscopic observation. The electrons lose, indeed, the character of distinguishable individuals. There is no longer any sense in the statement that one of two identical electrons is here and the other there, though there remains a sense in which one electron may be said to possess characters different from those of another. Individuality as such has gone: distinguishability of quantitative features survives.

On the basis of the modern mathematical laws, calculations of behaviour can be made, but the laws themselves are, in their present form, strangely unsatisfying to the minds of many. They seem almost to deny the possibility of the deeper kind of understanding. They present to the enquirer that austere negation which the philosophy of logical positivism seeks to impose in a wider sphere. Observable phenomena may, according to this grim code, be correlated but the kind of satisfaction which men of all centuries have looked for and called understanding must be renounced as a frivolous desire for something which does not exist.

Mind and matter in philosophy at large, individual ultimate particles in physics and chemistry, so far from being immutable essences become fictions providing illusory answers to improper questions. What a remarkable change from the excessively naïve and crudely mechanical conceptions of the early atomic theory.

But is this really to be the end, or does what is nearest to us loom at the moment in too large a perspective? Are science and philosophy really to be reduced to the filling in of forms about observables and the acceptance of the official answers?

So far from accepting this bleak doctrine, I think we may well stand on the threshold of even more surprising changes. If our present notions of particles and forces have involved us in difficulties and contradictions, then the plain inference is that we must go on thinking. From the nature of the case I cannot foretell what the resolution of the present dilemma will be, but there is one reflection on the subject which it may be well to make. A surprising amount of the structure of chemistry is

now seen to depend upon the famous Pauli principle, which may be said crudely to recognise different kinds of electron, but not different electrons. Behind this lies some deep mystery relating the possibility of detection with the very existence of the entities postulated in physical theories.

In a glass darkly one seems to see here some connection with the mind-matter mystery itself. All attempts to say something helpful about the relation of these two interpenetrating but apparently immiscible worlds has so far led to contradictions, frustrations and absurdities. The logical positivists in their cavalier way say that there is no mind-matter problem, but clearly there is one, and thoughtful people will go on wrestling with the question. Surely there is stimulus in the discovery that in physics and chemistry the things themselves which we suppose to underlie our observation of the world seem to have properties intimately conditioned by the possibility of observation. There certainly opens here no avenue to a facile idealist philosophy : not yet, it is permissible to feel, is the opening into a mere cul de sac. Perhaps rather it is the alluring entrance to a labyrinth through which a way exists for those who will be fortunate enough to find it.

The cycle of discovery, premature assumption of finality, disillusion and renewed endeavour is very characteristic of the scientific life. Easy predictions of unlimited progress are of little value, but in this strange region of which we have at least now become conscious, the goal lies very far ahead, but seems more worth striving after than ever. If we feel at times that advance has almost stopped, it is I think because we take a foreshortened view of the road already traversed. An immense conceptual gulf separates even the crudest of modern scientific ideas from the gropings of the ancient philosophers on similar subjects. May we even in passing—though the matter is one of little immediate practical concern—take a really long view and reflect on the gulf separating the most primitive ancient ideas from those of the ape ancester with whom Huxley was taunted at an earlier meeting of this Association. Before we conclude too readily that the limit of knowledge is reached we might do well to bear in mind that the human power of understanding may still have something in reserve.

Such reflections, valid though they may be, are somewhat remote from practical life, but the pragmatic conclusion is that we should continue patiently on the road.

Up to here these remarks have dealt with the fundamental theoretical aspects of chemical science. Although these deep and enthralling problems lie on its boundaries, much of chemistry is of a more workaday character, and for that reason less exposed to the hot and cold winds of exaltation and despair. But its closer relation to practical affairs lays it open to another form of attack. The gifts of science we are told, are in danger of being utilised not for the benefit but to the detriment of mankind, and voices are even heard which call for the restraint and repression of discovery. Men of science are sometimes represented as blind to higher values, and loosing upon an innocent world agents of untold destruction. This charge seems to me as grave as it is misguided, and in the course of my subsequent observations I shall take it upon myself to answer it.

But let us first look again at the major trends of chemical advance. The atomic and molecular theory, reborn beneath the murky skies of Manchester, provided the conception that atomic patterns underlie all substance and determine all properties. With what ingenuity our predecessors unravelled structures, visualising them first in plane projection, then bodied forth in space, discovering chains and rings of variously linked atoms in every sort of combination. All this is now of course the common tender of the subject.

Ever varying combinations of known and new methods continue to increase the wealth of knowledge about the chemical structures of all the compounds significant in Nature or Art, and now the aid of the mathematician is being invoked in the understanding of the stability and other properties of these manifold chemical systems. This theoretical technique is in its infancy. The tree may perhaps be said at the moment to sport gay blossoms rather than substantial fruit, but the stock is vigorous and the climate propitious,

and there is every hope of a good crop in the future.

In the great world of synthetic chemistry the chemist has not only learnt to discern the underlying patterns but to weave them largely according to his own designs—and we are sometimes asked whether this web is woven to good or evil ends. There are dyes which surpass Nature in variety and often transcend it in beauty, drugs which heal deadly sicknesses, polymers which adapt themselves in strength and flexibility to every diverse need. These and many other things follow from the chemist's knowledge of the architecture of the molecule. Even the subtlest agents of biochemical function, such as adenosine triphosphate, are being built up by his skill.

The same virtuosity, it is true, produces explosives of increased power, or poisons of greater potency. But can it seriously be laid at the door of the studious minority if the agents they produce to blast rocks are prostituted by the majority to blast one another? And in any case, though the precise calculation would be difficult, I should be prepared to venture the assertion that in the past century the good wrought by drugs, antiseptics and anaesthetics in saving lives and alleviating suffering far outweighs the evil which explosives and poison gases caused in wars. And speaking of the future it is at least a possibility that the control of cancer may emerge from the detailed chemical study of cell mechanisms.

Not less important than the knowledge of structure has been the understanding of the laws of energy, and the application of physical ideas to the interpretation of the intimate mechanism of chemical reactions. The molecular and kinetic theories, the discovery and interpretation of the laws of thermodynamics have not only illuminated our view of Nature, but have led to the rational understanding and control of all the varied processes on which chemical manufactures depend, and of all the applications and utilisations of energy.

Increasingly efficient machines, they are fond of telling us, are used for dropping destruction from the skies, but there is no scientific reason for this pastime. Nor have the works of man ever vied in destructive power with the vagaries of the Yellow River or the dire invasions of the great plagues. The beneficial uses of energy, analysed in Sir Harold Hartley's masterly address of last year, far outweigh the destructive. At the most primitive level they save us from death by freezing, and in more subtle ways the complex special fuels which the chemist now brings forth prepare the way for a unification of the world which is only impeded by vices of a kind having no connection whatever with science.

What is the power of destructive weapons compared with that of lying propaganda? And was this product of the devil begotten by a man of science? The inventions of applied science may attack men's bodies, those of seemingly humaner agencies can destroy their souls. To wish to inhibit or restrict scientific discovery is to show an utter lack of faith in human destiny. If there is any controversy about this, then it is not men of science who are blind to higher values, and in any case it would be preferable merely to be blind to higher values than to use an arrogant personal conception of them to stifle the pursuit of truth by others.

But to return to chemistry itself, one of the great waves of advance in the past century has been the detailed understanding of what actually happens in the course of chemical changes: how molecules collide and impart energy to one another, loosen their bonds, exchange their atoms, mutually induce electrical displacements, at times shed active fragments which create a sort of epidemic disturbance called a chain reaction, and at other times anchor themselves to surfaces where they enact their strange little dramas in a sort of Flatland of their own. The evolutions of atoms and molecules about which chemists now have precise knowledge are far more remarkable than those conceived by the poetic imagination of Lucretius, and this knowledge guides the manufacture of plastics, of dyes, and of every conceivable kind of chemical product. It is still advancing, and the first hesitant steps are being taken which transform an inductive into a deductive science. There is very far to go, but the journey is started.

In this matter of the mechanism of chemical reactions, I should like now to refer to the great field of the chemistry of the living cell, that wonderful skein of reactions out of which the life process is woven.

Chemistry, if not the father of life, is at least the godparent endowing it with its material substratum. In the last analysis the properties of the cell are determined by the molecular configuration of its substance, the arrangement of amino acid residues in proteins, the composition of the nucleotides, and the folding or piling of the chains and stacks which these various entities form. When a cell reproduces itself, protein chains must grow, like the polymeric molecules of the plastics industry. The nucleotide plates pile up, rather in the manner of crystal formation, but these two processes are subtly interlocked, and guide one another, as well as being in constant interplay with other reactions. The growth of a cell involves, as it were, an elaborate symphony of chemical reactions, the rules of which are slowly and surely being discovered. The chemical basis of the cell, with its function both in health and disease is thereby being gradually discerned.

What is dimly appearing includes the mutually aided autosynthesis of protein and nucleic acid, changes in proportions of enzymatic material in response to the change reaction velocities imposed by various environments, and discontinuous modifications in the molecular pattern caused by radiations or the accidents of abnormal cell division. The cell with the relatively stable structure of the molecular patterns which are the basis of its genes is a system of great traditional conservatism, but one which exhibits also response to change both of a long range and of a short range character, change imposed both by chance and by environment. Over and above this, we see the mass-mixing of chemical characters when cells undergo processes of sexual union.

The relative parts played by all these different effects are slowly being disentangled. The division of the cell itself, which is a necessity for the preservation of the type, is imposed by a physico-chemical influence known as the scale effect, and occurs without any doubt in response to a physico-chemical stimulus, about which more and more is gradually being discovered.

The selective influencing of cell processes occurs in many ways, and opens the door to the great practical field of chemotherapy. Chemotherapy itself is still in its infancy, and generations may well pass before it fulfils all its promise. There is ground neither for facile optimism nor for gloom, but simply a challenge to patient resolution. Growth must be long and difficult, because as yet the rational chemical basis for much that we know is incomplete. But the first dim outlines of it are certainly there.

At any rate the application of chemistry to health and healing is not likely to arouse the criticism of the layman except perhaps on the ground that it proceeds too slowly.

There is, however, a more sinister aspect even to these things. As the cell reactions disclose their secrets, as physiology advances, and as the relation of chemical structure to effect on cell and tissue clarifies itself, there will emerge the possibility of deep-seated chemical intervention into processes which are now normally inviolate. Chemically induced mutations of cells are already known in a crude fashion, the influence of drugs on personality already exercises medicine and law, and the day may well come when a conscious moulding of individuals and even of races will present problems of fearful fascination.

If this day does indeed dawn the sky will ring more wildly than ever with cries against science, and the old battle of ultimate values will be joined more vigorously than ever. But it will still be those of little faith who fear the conscious intervention of mankind in the fashioning of its own destiny and who oppose what could equally well be represented by those so minded as part of a great purpose.

As we pause to gain a second breath at this mid-century we are assailed by complex emotions. The distant prospect thrills, the clouds of pessimism are transient, the flames of opposition die down like Wotan's magic fire before those who are not afraid. But the scale of effort and endurance demanded seems to exceed the conceptions of the past. What then must we do?

There are two aspects of this question, the scientific and the human.

Practically, an immensely important task of the man of science in general and of the chemist in particular is to protect himself against two very present menaces, the one of being controlled by obscurantist

critics, the other, much more dangerous, of being smothered and frustrated by those who may think they wish him well.

The man of science is in peril of being swept away into a Sargasso of administrative seaweed. He is constrained to forsake the laboratory for the conference hall and board room. Endless trivial reportings are displacing learned writing, petty shifts consume creative energy, and the whims of bureaucracy and doctrinaire accountancy more and more usurp the place of liberal management. This has already gone so far that a temporary decline in fertility in the near future is almost inevitable, but with resolution and skill we shall come through.

On the scientific side there has grown up a thicket of specialised detail with a loss of wider perspectives. This, too, is temporary and will pass as broader syntheses are constructed, and as new and more powerful theories succeed further in the tasks of co-ordination and prediction.

If in face of all these hopes and fears, doubts and assurances, we ask for a policy, there is no principle more pertinent, more sane or more necessary than that of Voltaire's hero, Candide, ' Il faut cultiver notre jardin.' If the chemist follows this homely advice and unconcernedly cultivates his own, it will not fail to go on producing its flowers and fruits in undiminished profusion.

THE INFLUENCE OF GEOLOGY ON MILITARY OPERATIONS IN NORTH-WEST EUROPE

Address by

PROF. W. B. R. KING, O.B.E., M.C., F.R.S.

PRESIDENT OF SECTION C

WHEN we look back into history, we see that the type of military operations in north-west Europe changes from time to time for various reasons, among which are primarily the location of the major centres of military strength and the kind of weapons used. Nevertheless, there appear to be two great overriding considerations which have influenced military operations ever since large armies were employed. Firstly the availability of communications, which allowed the movement of troops and equipment and made it possible to feed and clothe the army, and secondly the configuration and state of the ground, which controlled the actual deployment of the opposing armies in battle. Before railways were built, road and barge traffic shared in the problem of transportation, and fortifications were placed so as to control and command the few main roads and water transport systems. The reduction and capture of fortresses was not so much a military objective in itself as a means of freeing lines of communication, thus allowing the army to manoeuvre for position for the battle which followed as the main opposing forces came to grips. We need not go back many centuries to study the way the theme I am taking works out, but from the time of Marlborough onwards the pattern of operations and the control exercised by the geological features becomes clear.

The campaigns of Marlborough, apart from that which culminated in Blenheim, were mainly fought in Flanders and the adjacent areas of Brabant and northern France. Although the reduction of fortresses constituted an important part of each year's campaigning, Marlborough's great victories were won by skilful manoeuvring leading up to pitched battles in the open. In these, ability to move large bodies of troops was clearly influenced to a very marked degree by the position of naturally strong defensive river lines and the gaps between them, or possible crossing points not covered by a fortress.

Perhaps the most striking example of topographical controls affecting strategy is that of Ramillies, while Oudenarde and Malplaquet are further examples. The great defensive lines of French fortifications, first at La Bassée and then the 'Ne plus ultra' lines, illustrate cases at this period where natural features were strengthened by inundations and artificial earthworks to block movement from one area of open country to another. That they did not stop Marlborough is a testimony to his military skill rather than any indication that these were not naturally strong features based on geological considerations and were not the correct places for siting defensive lines. Rather do they illustrate that in 1711 as in the wars of 1914–18 and 1939–45 strong defensive positions, though obviously better than weak ones, cannot stand against first-class generalship combined with the will to break the position and backed by adequate equipment and men.

With the development of artillery the close-walled fortified city became of less and less importance and fortifications developed into a series of forts which were in the main protection for gun emplacements. Napoleon was a master in mobile

warfare and it is significant that the great Napoleonic battles were in open country. The same may be said of Wellington. In the area we are considering these two did not meet face to face till Waterloo and the site of the battle is in the centre of this area.

In 1870 there was a change in the place of attack, the German columns descended on Sedan, avoiding Belgium, but in 1914 there was a return to form and the first battles followed a break through along the 'corridor of invasion,' across the 'cockpit of Europe,' spreading out over the open chalk lands of northern France with thrusts directed at Paris.

We come now to 1939 with Belgium and Holland neutral and with the French and British separated from the enemy over much of the front by great distances. In May 1940 Germany invaded the Low Countries and Belgium, whereupon the French and British, at the request of the Belgians, entered Belgium. During the previous winter this eventuality had been expected and plans had been worked out. Three possible lines of defence were considered : the most easterly line was the frontier of Belgium and Germany, and it was unlikely that the allied forces could reach this line in time ; the next, the 'D' line, ran from Antwerp using the line of the River Dyle (hence the 'D') across the 'corridor of invasion,' to Namur thence following the line of the Meuse till it reached the Maginot Line ; the more westerly line was that based on the River Escaut (called the 'E' line). I hope to show from a study of the geological controls that the 'D' line was the natural and obvious line to try to hold and this was the one which the allies took up in May 1940. Things, however, did not go as predicted and the flank of the position on the Dyle was turned in the south, and while a stand might have been made on the Escaut (the 'E' line) this again was turned by the rapid advance towards Arras and the coast. Withdrawal from Belgium was perforce necessary and the evacuation from Dunkerque took place.

A new line, the Weygand line, was for a time stabilised, based on the Somme, following the edge of the wooded plateau of Laon (formed by the Tertiary strata of the Paris basin) and running thence east to join the Maginot Line—again a line of strong natural defence which we shall see is controlled by the geology of the area. However, with the cracking of the Weygand line at Abbeville, again at a point where geology exerts a controlling influence, the whole front became fluid and the enemy overran the rest of the French Army.

The story of the 1944 invasion from this country across the Normandy beaches illustrates again the importance of a number of geological controls.

Let us now examine the general geological pattern of north-west Europe and see where the oft-repeated pattern of military operations can be co-ordinated with the physical geography of the area and the way in which this is clearly controlled by the geological history. The area falls into five natural regions :

(1) The low-lying area of north and north-west Belgium and the Netherlands formed of late Tertiary beds generally covered by alluvium or peat.

(2) The chalk and high-standing Tertiaries with inliers of Palaeozoic rocks forming an east-west belt north of the main mid-Meuse in Belgium and stretching away through northern France past Lille to Boulogne.

(3) The Palaeozoic and igneous rocks of the Ardennes and the Vosges with the intervening area of the Sarre and the hill country of Bunter sandstone.

(4) The main Paris basin in its larger sense, which can be divided into (a) the outer belt of the Trias, Jurassic and Lower Cretaceous strata, (b) the middle zone of the Chalk with the open Tertiary area of the Beauce, and (c) the central area of the Tertiary mass round Paris itself.

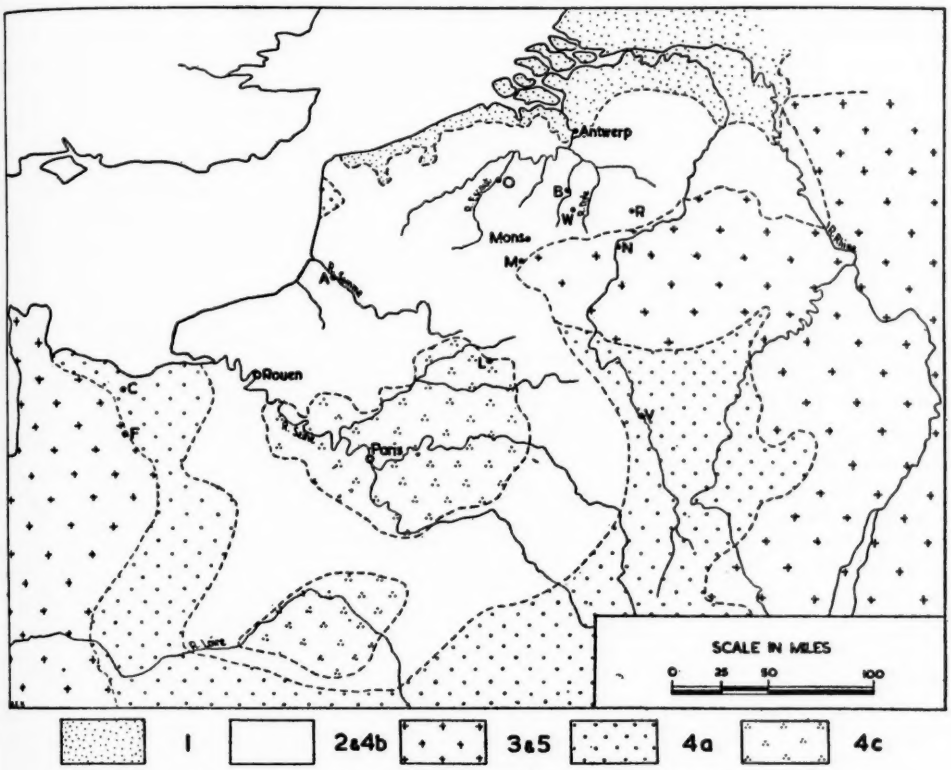
(5) The Palaeozoic area of Brittany and west Normandy.

Besides this lay-out of the 'solid' geology, the fact that the area was in the periglacial zone, lying immediately beyond the ice-front during the various ice advances, had far-reaching results on the final geography of the area.

It is not my intention to go into the details of the tectonics and stratigraphy of the various divisions but it is necessary to point out a few of the special characters of each subdivision.

(1) The low-lying areas in the north. These are formed of soft alluvial deposits

Map of the natural regions of North-West Europe



Numbers refer to the divisions noted on the opposite page.

Towns (West to East)

C = Caen; F = Falaise; A = Abbeville; L = Laon; O = Oudenarde; M = Malplaquet; B = Brussels; W = Waterloo; N = Namur; R = Ramillies; V = Verdun.

with wide, deep sluggish rivers and numerous canals, clearly making cross country movement difficult, while much of the area, being artificially drained, is therefore capable of being flooded. These conditions clearly aid defence and inundations if conveniently located, form parts of naturally strong defence lines. As an example of this, the flooded areas of the Yser in 1914–18 formed a strong defensive flank to the Ypres salient.

(2) The Chalk and high-lying Tertiary tract of Belgium and northern France. The fact that the area is high-standing and possesses a fair relief means that in summer it is well drained, but the clayey nature of the soil, due mainly to thick superficial deposits, results in wet slippery surfaces during winter and spring. However, particularly in the southern part of the area, we are near the watershed of the rivers converging on Antwerp on the one hand and those draining towards the Meuse and its tributaries on the other. This results in there being an east-west line where there are few major physical barriers, forming a natural east-west corridor for free movement.

(3) The Ardennes, Vosges and intervening area. These are either plateau lands with deeply incised valleys or steep mountainous country often heavily wooded. They form a belt of country on the east of France and southern Belgium which by its nature is extremely difficult country for the movement of large bodies of troops with heavy equipment, and when fortified in the modern manner of the Maginot line was considered impregnable. The weakness obviously lay in the fact that the boundary between France and Belgium divided this natural unit between two

countries, which precluded a joint plan for the proper layout of a continuous defensive system. Nevertheless, the area as a whole favoured defence, particularly along such lines as that where the Meuse cuts across the strike of the beds south of Namur in a deep, steep-sided gorge.

(4) The main Paris basin. The general geological arrangement of the Mesozoic and Tertiary strata is extremely simple, being that of a number of saucers of progressively smaller diameter 'nested' one inside the other. The main complication is the failure of the Jurassic and Lower Cretaceous outcrop between the western end of the Ardennes and the Boulonnais. Thus from the Normandy coast round the southern side of the basin to the Ardennes we find the strata dipping gently inwards towards Paris, and since they consist of alternating limestones and clays in groups of strata each a hundred feet or so in thickness, there develops a pronounced dip and scarp topography the like of which, for regularity and persistence, is seldom found save in our own English Mesozoic rocks. The military significance of this is considerable, for each scarp overlooks the long gentle dip slope on its outer edge and, whereas the scarp edge of limestone is well-drained, the foot of the scarp and lower parts of the dip slope are liable to be wet and swampy clay lands. The configuration therefore tends to favour defence against anyone moving towards the centre of the basin. In our summary we divided the Paris basin into three sub-zones. The dip and scarp feature is best seen in the Jurassic and Lower Cretaceous belt, where many of the clay belts are heavily wooded and only the limestone tops are open country. In the middle zone, however, we find the Chalk forms large tracts in part covered by loams but in part bare Chalk downland, while into this zone may also be put the Tertiary lands of the Beauce where open well-drained country is as characteristic as in the Chalk areas. These open areas are the next best thing to the 'desert' for open tank warfare, particularly in summer and early autumn when the superficial loam covering, where it exists, is dry and firm. This country is also ideal for the rapid construction of fighter airstrips. The innermost group is formed of practically flat-lying Tertiary deposits of massive limestone alternating with soft sands and clays. The result in a general way is a well-defined steep scarp edge surrounding an area of diversified woodland with well-entrenched valleys.

(5) The remaining division is the Palaeozoic massif of Armorica. The marked east-west strike implants a strong 'grain' on the country which is seen in the elongated ridges of high ground where massive beds of resistant rock, such as quartzites, stand up as steep-sided ridges, as near Falaise, restricting north-south movement. The effect of this was seen in the way the United States forces, once to the south of the ridge of high ground at its western end, were able to sweep eastward with such speed along the strike of the softer beds.

Although the solid geology clearly has a great influence on military operations, the area we have been considering was subjected to all the effects of periglacial erosion and deposition during the Pleistocene Ice-age. In analysing the effects of this periglacial position, we note that in the first place there is abundant evidence that during the maxima of the ice advances there must have been a marked eustatic lowering of sea-level, and in general it is agreed that in the area of the English Channel and northern France and Belgium isostatic movements would not be expected to be significant. This area was possibly even in a zone of compensatory up-bulge around the main Scandinavian depressed area. We may with safety visualise a time when all the rivers, at any rate in their lower reaches, were undergoing marked rejuvenation. Whether the upper reaches of the longer rivers may not have been suffering from aggradation by excessive solifluxion need not concern us here. The point that is of importance is that the lower reaches of the Seine, Somme and all the rivers draining to the estuary of the Scheldt must have been markedly rejuvenated and must have incised their beds to a marked degree into the relatively soft Mesozoic and Tertiary rocks. This may have happened several times, for during each low sea-level period the rivers easily and quickly would clear out any deposits which had accumulated in the drowned valleys when water-level rose in the interglacial periods. Clearly therefore

the effect of the latest lowering and refilling of the oceans, resulting from formation and melting of large ice-caps, left the most obvious results in the lower river valleys and coastal reaches. The 'Flandrian Transgression' has been responsible for the swampy, peaty infillings of so many of the lower river valleys and some coastal areas.

Whether the lower parts of a river valley became a marine estuary or merely a peat-filled fresh-water swamp appears to depend on the quantity of material brought down by the river at the time of rising sea-level. In some of the rivers such as the Seine there appears to have been sufficient gravel and sand in transport to prevent any peat swamps forming and so, although large alluvial and gravel flats occur, particularly where refilling the excavated deep-sided valley, the only real military obstacle over great lengths of the valley of the River Seine is the river itself.

Very different however is the case of the Somme valley and to nearly the same extent the Lys, Escaut, Dyle and many others. In these valleys the rivers flow through low-lying fen-like swamps from which peat has frequently been dug. The river itself is only a very small part of the actual obstacle from the military point of view. In the case of the Somme in particular the crossing points are few in number and are generally situated at points where river gravels have been brought down from tributary streams or where calcareous tufa has built up a deposit firm enough to carry a roadway. We thus see why the northern river lines are natural defence positions apart from the actual river itself.

The strength of the middle Meuse line above Namur depends on the fact that the river there is incised into an old uplifted plateau of hard rocks resulting in a gorge with almost precipitous sides, a line of great natural strength which should be able to be held by very few troops.

We must now consider certain features which have resulted from the very special deposits which formed in the periglacial area surrounding an ice-cap whose outer margin is on a relatively flat-lying land area. The meteorological conditions round the margin of the Scandinavian ice-cap have been considered by several workers and, whilst some claim that a glacial anticyclonic type of air circulation must develop with strong north-east winds in the periglacial area, others claim that the normal westerly circulation must have been maintained to nourish the ice-cap with moisture-laden winds. When speaking of the winds under the periglacial conditions pertaining at the time, it would perhaps be more accurate to call them 'moisture-bearing' rather than 'moisture-laden.' What is demanded is that they should have enough moisture to give precipitation when forced to rise and become chilled by the high surface of the ice-cap.

It must also be remembered that at the glacial maximum large tracts now covered by the sea had been laid bare and would probably be in a state ready to supply considerable quantities of dust to the winds whichever way they were blowing—a point made by several French authorities. However, whatever the meteorological conditions were, it is clear that the loess was deposited over much of the area we are considering. The characteristics of this deposit are of the greatest importance from the military point of view. Since it was wind-carried, it was spread as a great blanket over hill and vale alike but during the subsequent periods of greater rainfall it has been washed from the slopes and is now found only on the plateaux or partially filling the smaller side valleys. In its original state it is a fine calcareous loam with grains mainly of the silt grade. In common with nearly all loess deposits it has a tendency to a rough vertical jointing structure which allows it to stand almost vertically in open cuttings and, during dry weather to drain well, forming a firm even surface. When weathered it develops a 'loamy weathering' largely by decalcification and then the surface behaves rather more like a clay. But even so, on drying it does not shrink and crack in the same way as a true clay but when wet the weathered loess 'puddles up' to a fluid mud which becomes almost impassable to concentrated or heavy traffic. The loess-covered plateau has another feature—the soil produced on these surfaces is an excellent arable soil, and in France and Belgium forms the main plough lands, being cultivated in huge open fields without

hedges or ditches. When dry, therefore, we find the vast almost horizontal surfaces of the plateau regions formed of firm, dry loam land, ideal for the siting of temporary airstrips and for rapid and uninterrupted movements of armoured vehicles. However, when the soil of these areas becomes soaked with water in the winter and spring months, they cannot be used for these purposes, certainly not for temporary airstrips, without considerable preparation and surfacing. In fact for most cross-country movement the area must be downgraded to the same category as pure clay lands. In most of the areas of chalk, the covering of loess overlies thick clay-with-flints which produces a much more intractable soil and so, if there had been no loess formation at some time in the past in this area, we would have had a very different agriculture. This would probably have been of the ' bocage ' type with its numerous small fields, high hedges and orchards, rather than these great open stretches of country.

As far as the land areas are concerned the main effects of the glacial period in the area we are considering from a military point of view were therefore, first, the formation of overdeepened valleys later filled with peaty alluvium forming well-defined transverse barriers, and secondly, the even covering of loess giving wide open spaces ideal for free movement during seasons when the soil was dry. This is particularly important in area 2 and to some extent in area 4, for where the solid geology gave a flat plateau topography, the loam covering gave an agriculture of large fields without hedges. In the inner zone of the Paris basin this advantage is masked by the deep dissection of the area, while in the two areas of old rocks the loess cover is usually absent or very sporadic.

On the coasts however we find another complication resulting from the movements of sea-level in glacial and post-glacial times. The effects of the Flandrian Transgression are well seen in the type-area of the Flanders coast lands but it is the latest oscillations of sea-level as they affected the coasts of the Channel and North Sea that produced certain outstanding conditions and deposits. At first sight a study of this movement of sea-level might be thought to be of purely academic interest and of no military significance. Let us look at it from the scientific standpoint first and see what can be deduced from the known sequence of events.

There is the generally accepted figure of 100 metres for the lowering of sea-level during the maximum of the last glacial episode but we need not argue about the exact figure. All that is relevant for the present argument is that there was an eustatic lowering of sea of about this order of magnitude. This must have resulted in a complete draining of the North Sea basin as far as the Dogger Bank and the English Channel to a point about the longitude of the Lizard, with perhaps an estuary running up the line of the Hurd Deep (Fosse Central). But apart from this time of extreme lowering, the sea-level must have been considerably lower than at the present time in late glacial and immediate post-glacial times. During this time the older beach deposits would be eroded and deep channels cut through the hard solid rocks of the former off-shore wave-cut platform where this consisted of limestone or other resistant rock; the valleys themselves would be deepened; new sea-beaches or coastal dunes would form and behind these, swamps and fen peats would develop, resting either on the remains of the older beach deposits or on the now exposed wave-cut platform of solid rock. As the sea-level reached its present position, the old peats and forests were submerged and the present beach developed, beach sands overlying the peats in those places where the invading sea did not remove them by erosion. A new pebble beach would develop at and above the new high-tide mark formed of stones, either driven landward by the sea or derived from denudation of the cliffs. Thus we find many of our own coasts and those of the French side of the Channel characterised by off-shore stretches of a wave-cut platform of solid rock with here and there deep passages cut through it, then, landward of this, patches (often extensive) of plastic clay and peat overlain by a veneer of modern beach sand of variable thickness and, finally, a storm beach of pebbles behind which the modern fen-like deposits are now forming.

It was these conditions on the Normandy beaches which gave our planning staffs such concern, since experiments on counterpart British beaches had shown that most vehicles could not travel over outcrops of peat or plastic clay and were even liable to cut through a thin cover of sand and become bogged in the underlying soft deposits. It was known from French records that peats existed on the Normandy beaches but in the early days of the planning, the beaches appeared from air photographs to be evenly covered by sand. After the storms of 1943, however, suspicious dark patches in the bottoms of the runnels suggested that peat might be exposed. Much careful work over the next six months culminated in the preparation of large-scale maps of the beach indicating those parts which were unsuitable for the passage of assault vehicles, and a pre-invasion reconnaissance by commando volunteers confirmed the existence of outcropping peat. The success of the landings was sufficient justification for the time spent on this problem. By far the greater part of the geological research was carried out by Professor Shotton who was then geologist at H.Q. 21st Army Group. The deep-water entrance to the 'Mulberry' harbour was the result of the erosion of a gap through the old wave-cut platform during the time of low sea-level, so that again we see the control exercised by the geological history of the area.

The presence of flat-lying Jurassic limestones covered by loess-like loam made it possible to construct, with remarkable rapidity, the necessary airstrips, for here we had the nearly ideal conditions of a permeable rock at depth stripped by weathering to a flat topography and covered by a loess-like loam which during summer months dried quickly to a firm soil. Owing to the lack of irregularities such as ridge-and-furrow or even hedges scarcely any work was necessary on levelling the sites. These airstrips allowed the fighter aircraft to be serviced near the frontline thus helping materially to maintain the air superiority with all that that entailed.

Had the return journey across France and Belgium been contested stage by stage, no doubt the geological controls mentioned previously would have operated but after the Battle of Falaise things moved so quickly that geology did not enter into the problems until the front was temporarily stabilised in Holland and at the German frontier.

The object of this address has been to point out the ways in which the geological history of an area has controlled the military strategy and produced a pattern which tends to repeat itself on successive occasions. With changing military arms and equipment the details change but the basic pattern remains remarkably constant.

ORGANIC DESIGN

Address by

DR. C. F. A. PANTIN, F.R.S.

PRESIDENT OF SECTION D

WHEN the Council did me the honour of inviting me to be President of Section D this year, it was suggested that it might be of interest to review some of the contributions of British men of science to zoology during the last 100 years. This would certainly be a good occasion to attempt such a task. For the period begins with the work of our greatest pre-Darwinian comparative anatomist, Richard Owen, to whom even the names for the bones of our vertebrate skeleton are a memorial; and only eight years after 1851 came the scientific revolution of the *Origin of Species*.

But even if we took only a selection of the great figures in the field of traditional zoology there would be too much to say. To do justice to the work of men like Thomas Huxley, Balfour and Ray Lankester down to Goodrich in our own time would need much more than a single address, and it is already done in Russell's grand survey (36). Nevertheless there is one theme of peculiar interest at the present time: the significance of organic form. Pure morphology was the dominant feature of Victorian zoology. The revolt against it which broke out after 1918 led to all manner of functional studies on the living animal, in many of which structure seemed of little importance. But in the last twenty years almost every one of these has led back again to the importance of structure, though from a very different aspect from that of evolutionary morphology. How this came about is worth our attention.

By the beginning of the nineteenth century two principles had begun to emerge from the study of comparative anatomy. First, that structures were adapted to the function they served in the organism; second, that quite apart from function, some plan underlies the architecture of whole groups of animals so that the same characteristics recur again and again in different species.

The idea of functional adaptation is well recognised in common speech:

> 'Beware the Jabberwock, my son!
> The jaws that bite, the claws that catch!'

Its significance has long been appreciated. Just before our period, Paley, the theologian, used it when he developed the powerful argument of John Ray [1] for the existence of a Deity who designed all living creatures. For just as the functional adaptation of the parts of a watch to the purpose for which they are intended allows us to infer the existence of a watchmaker, so, he suggested we might likewise infer from the purposive adaptation of the parts of the human and animal bodies the existence of a Grand Designer.

I should like to give you an example of this apparent design from amongst the simplest of the cellular animals. In the sponges, the body consists essentially of a porous cylinder of loosely organised cells. Through these pores are swept the fine planktonic organisms upon which the animal feeds. Certain cells construct a skeleton which may be of protein or of calcium carbonate or of silica. Among the siliceous hexactinellid sponges there is one of singular beauty, *Euplectella aspergillum* of Owen. Its skeleton is a hollow cylindrical framework of siliceous spicules. These are arranged in the form of longitudinal struts running the length of the cylinder, which are braced by lighter hoops at right angles to them at regular intervals. These simple components of themselves confer great mechanical rigidity

[1] Though it was Paley's version that influenced Victorian thought, we might use the stronger verb 'plagiarised.' Raven (34).

on this seemingly delicate structure. They can evidently withstand compression in the same way as in the steel framework of a building. By itself such a structure would have little power to resist a twisting force. Mechanically such torsion can be met by the insertion of ties running obliquely through the rectilinear network of struts. In *Euplectella* we find this very thing. We find that in addition to the firm system of a rectilinear siliceous scaffold there is a network of thinner siliceous ties running in spiral geodetics through the framework. In fact we arrive in this skeleton at a marvellous combination of rigidity and lightness which recalls the geodetic construction familiar to aeroplane designers.

It seems impossible not to suppose that this skeleton is adapted especially to give lightness and rigidity. In the skeleton of *Euplectella* we infer its adaptive nature entirely from what we see in the dead skeleton. The animal lives at great depths in the Pacific off the Philippine Islands. No one has seen it alive in its natural habitat. We judge the adaptation in this case because the structure is organised on a special plan which we know from experience has special mechanical properties, in the same way as we infer the function of the claw of a lobster which is washed up on the beach even though we never saw the lobster to which it was attached. But, as Richard Owen showed, we may be misled if we too easily ascribe all morphological features to purposive adaptation. The ossification of the bones of the skull in placental mammals to form separate centres may appear to be designed to facilitate the birth of the offspring. Yet, as he says, ' something more than such a final purpose lies beneath the fact that most of these ossicle centres represent permanently distinct bones in the cold-blooded vertebrates ' (27, p. 73). The pattern of ossification in the developing bone is the same, but we cannot in that case apply the same teleological explanation. The fact is that purposive design like beauty, to which it is very closely related, is to some extent in the eye of the beholder.

We now come to the second principle of animal morphology, the existence of a limited number of patterns of architecture ; the vertebrate, the arthropodan and so on. We do not find every sort of organisation of which the mind can conceive. Two features were discerned in these underlying patterns : one, that animals could be placed in some sort of series ranging from higher to lower grades ; the other, that animals could be grouped into types. The first notion had long received expression in the idea of the Scala Naturae, in which the whole of animated nature was placed in a series with man at the top, grading down to inanimate objects in a linear scale. The inadequacy of this conception was fully realised at the beginning of the nineteenth century. Cuvier showed how at best it could only be applied in a limited way, and that animals fell into a number of distinct classes which could not be placed in series. But this idea of a scale dies very hard. Particularly with human physiologists we often hear the phrase ' lower in the evolutionary scale ' as though invertebrate animals were merely so many stages of increasing simplicity of organisation leading down from man and the mammals. In part this attitude arises from an anthropomorphic tendency to place man at the head of creation. But the error persists because it contains an element of truth which is forced upon the attention of those engaged in functional studies. If, for instance, we compare a mammal with a sea-anemone, we have two animals both of which meet the requirements of their existence with efficiency. But there are important differences in the way their structural organisation permits them to do it. A sea anemone consists essentially of a muscular sac surrounding a volume of fluid which combines the offices of a digestive fluid and of a hydrostatic skeleton. Local contractions of this sac necessarily have very complicated mechanical consequences : it is not possible for the anemone to execute the peristaltic movements of digestion without also affecting every other activity, such as locomotion ; nor can it move its body without affecting its digestive fluids. In the mammal, the muscular, digestive and other systems are functionally differentiated into well defined and separated systems of organs. This functional separation endows the mammal with much greater capacity to

meet the demands of a more varied environment than that of the sea anemone. In particular, such differentiation allows the development of homoeostatic mechanisms, such as those which control the composition of the body fluids.

It would be a mistake to suppose that this functional differentiation only involves increasing complication of gross structure. On the contrary, a most characteristic feature is secondary simplification; a simplification which is neither primitive nor degenerate. The mammalian circulatory system is simpler than that of the frog. That admirably simple device, the reflex arc, is developed independently in all sorts of animals to solve certain simple response problems; just as an ingenious mechanism may simplify a process of manufacture. But this simplification on the 'output' side can only be attained at a price; these functional devices require increased central control. The structural complication of the brain is not so visible to us as the complication of the other organ systems. But this great complication of the computing and predictor mechanisms which control animal behaviour is characteristic of what we call 'higher' organisms. It is a price that must be paid for the development of functional devices which free the animal from dependence on the environment. As in other cases, the road to bureaucracy is paved with good inventions.

Apart from the possibility of being able to arrange organisms in a series of higher and lower grades of organisation, it was fully recognised at the beginning of the last century that groups of animals were organised on one or other of a limited series of architectural plans, such as that of the Vertebrata, the Arthropoda, the Mollusca and so on. Precise definition to this idea was given by Owen in his concept of the archetype. If we consider two vertebrates such as a mole and a bat, we see that although in these two cases the forelimbs have entirely different functions, yet the skeletal plan of the limbs is in both cases the same. It was Owen who gave us the definition of homologous organs as the same organ in different animals under every variety of form and function, differentiating this kind of resemblance from that we find in analogous organs—a part or organ in one animal which has the same function as a part or organ in a different animal.

Analogical resemblance at first sight seems trivial; the mere resemblance of the claw of the lobster to the hand of man because they are used in a similar way, although they possess no fundamental structural similarity. On the other hand, we frequently find in the animal kingdom organs which show remarkable similarity of structure even though they are to be found in animals whose bodies are otherwise based on totally different plans. A famous example of this is the molluscan eye. The eye of a cuttle fish mimics the eye of a vertebrate. In both cases we have the same arrangement of lens, iris, retina and so on. It is true we find differences in the innervation of the retina, but the resemblance is striking. Indeed, in the eye of *Pecten* we have even an inverted retina. This was one of the cases which led Bergson to say 'whether we will or no we must appeal to some inner directing principle in order to account for this convergence of effects' (2). We shall return to this question later, but it is, I believe, an extremely important one, for recently similarities of this same nature have become evident in what are perhaps the most important structural features of the most important organ of all, the brain.

By the middle of the nineteenth century it was clear that the phenomena of adaptation, of homology and analogy required some explanation. The possibility that such features might be of evolutionary origin had already been suggested by Lamarck. But the extravagance of his many assumptions made his hypothesis vulnerable to the attacks of Cuvier and Lyell. In 1851 the views of Owen were on the whole scientifically sounder than those of the evolutionists on the evidence then available. He held that in each of the main phyla there was an underlying plan or archetype built up of a series of similar parts, such as the vertebrae of a vertebrate, the segments of centipedes and worms and the ossicles of Echinoderms. These parts were then modified in different species in adaptation to their special conditions of existence. Thus the limbs of animals of the same type though adapted

to different modes of life, showed a homologous structural plan. The simpler the grade of animal organisation the more is mere 'vegetative repetition' of parts in evidence, and the more do these approach simple geometrical figures. Finally as in starfish and sea urchins, the skeleton actually betrays its calcitic crystalline nature; the rules of organic form approach those of the inorganic world. On the other hand, in the higher animals this simple repetitive plan becomes increasingly obscured by the adaptive specialisation of the parts (27, p. 171).

Both adaptation, and the underlying plan exhibited by the archetype, were, in Owen's eyes, evidence of divine design. Agreeing with Paley he says, 'With regard to the adaptive force . . ., its effects must ever impress the rightly constituted mind with the conviction that in every species "ends are obtained and the interests of the animal promoted, in a way that indicates superior design, intelligence and foresight in which the judgement and reflection of the animal never were concerned; and which . . . we must ascribe to the Sovereign of the Universe" . . .' (27, p. 172). And the homologies of the underlying plan we must surely consider as 'manifestations of some higher type of organic conformity on which it has pleased the divine Architect to build up certain of his diversified living works' (27, p. 73).

But these theological conclusions were not essential to Owen's argument. At that time it seemed perfectly legitimate to infer that the peculiar laws discerned in organic form were expressions of fundamental properties of the material universe like those which govern the different forms of crystal structure. Such a notion was not just metaphysical speculation. We see evidence of an underlying plan of this very same kind in the properties of the chemical elements. The relations of a family of different species of animals is not at first sight so fundamentally different from the relations of a family of elements, such as Lithium, Sodium, Potassium, Rubidium and Caesium. And though various evolutionary explanations were from time to time given of the occurrence of the elements—like that of Sir William Crookes at the 1886 meeting of the Association—the sequence of related properties in the alkali metals still seems ultimately to depend on the unique properties of matter as it occurs in our universe.

Owen's view had some important consequences which he himself recognised. If animal structure represented the development of an ideal plan, we might expect the same fundamental types to recur even on other planets, if conditions happened to be suitable for the maintenance of life: "But the laws of light, as of gravitation, being the same in Jupiter as here, the eyes of such creatures as may disport themselves in the soft reflected beams of its moons will probably be organised on the same dioptric principles as those of animals of a like grade of organisation on this earth. And the inference as to the possibility of the vertebrate type being the basis of the organisation of some of the inhabitants of other planets will not appear so hazardous, when it is remembered that the orbits or protective cavities of the eyes of vertebrates are constructed of modified vertebrae' (28, p. 83). We have by no means discarded such views to-day. With the authority of some biologists the author of a brilliant and recent popular discourse on the nature of the universe has arrived at just such a conclusion (16).

But in 1859 the whole of biology was transformed by the publication of Darwin's *Origin of Species*. Nothing is more remarkable in the history of science than the speed with which Darwin's work caused the theory of evolution to be generally accepted. Himself a naturalist and systematist, the first effect of his work was overwhelmingly upon abstract comparative anatomy. His theory did not contradict the generalisations of the anatomists. On the contrary, it gave elegant formal explanations of them. Much the most important conclusion was that the mechanistic operation of natural selection could produce purposive adaptation. Purposiveness had supplied the teleologists' main argument in favour of an extra-physical or divine directive process governing form. Now it became, as it were, the negative image of blind environmental forces. Other generalisations also received an explanation, at least in part. The underlying structural

plan of the archetype became the structural plan of the ancestor, which had undergone various adaptive evolutionary modifications in its descendants. Owen's homology was in fact the inherited result of ancestral adaptations. The ridiculous little second and third toes on the hind feet of kangaroos and of bandicoots are of no adaptive significance to them, but they indicate common descent from an ancestor in which reduction of these toes was once an adaptation to climbing trees. And whilst homology was just what would be expected in the varied evolution of parts derived from a common ancestor, analogy could be explained by convergence of different forms under the same forces of environmental selection.

Under the influence of evolutionary theory, the study of abstract comparative anatomy made immense advances during the next 60 years. It dealt particularly with the hunt for archetypes in the guise of hypothetical ancestors. Despite the importance assigned to adaptation, the relation of form to function received little attention. Till 1918 Charles Kingsley's 'Professor Ptthmllnsprts' held the field. But now the study of form no longer brought spectacular advances like those of the eighties, and after the appearance of that magnificent landmark Sedgwick's *Text Book of Zoology*, zoologists turned to a dozen new lines of research far removed from traditional comparative anatomy.

There were many quite new developments. Unlike botany, where physiology and anatomy have always developed side by side, the zoologist had left the study of function to the human physiologists—whose view was necessarily one-sided. But now it was felt that comparative physiology was as important to the zoologist as comparative anatomy. Moreover genetics was taking its modern shape; and the behaviour of animals and the study of ecology attracted more and more research. There was in fact a period of intense activity connected with study of the living animal; and—as anyone who tried to lecture on those fields in those days will know—it seemed difficult or impossible to find a continuous thread running through all these activities. There was a curious mixture of enthusiasm in research and sad disillusion about the unity of the science.

But at least in principle it might still be possible to study the structural relationships of animals by an extension of the older methods. The nineteenth century zoologist was limited by his scalpel and his microscope to the relatively gross anatomy of the tissues. In the 1920's, the rapid advance of physical chemistry put into the hands of the biologists new methods of examining structure, particularly at simpler levels; those of the cell, the colloidal system and the molecule. The relationship between two organisms depends on the number and quality of points of resemblance between them; and these new methods certainly brought to light new relationships. But these were quite of another kind from those discovered by the evolutionary anatomists.

To appreciate the character of these new discoveries, it is necessary to realise that the new outlook was vigorously mechanistic. There was a praiseworthy distrust of authority in favour of direct observations and concrete physical models in explanation of them. But the use of such models carries some danger of oversimplification, and the underlying assumptions of much of this work were often curious: particularly those concerning the cell. How does the cell work? Data were collected from vertebrate nerves, algae, amoebae, and the eggs of sea urchins. Bearing in mind all that evolutionary divergence implies, what right was there to suppose that the few known points of structural similarity between these very different objects would justify the assumption of far-reaching physiological similarity: that experiments on the permeability of a species of amoeba could throw any light on the organisation of a sea urchin's egg? The logical basis of such work is by no means easy to justify. Yet the fact emerged that in practice, justified it was.

Already at the molecular level strange new relations appeared. By 1915 the work of Harden & Young on the production of alcohol by yeast, and of Embden and others on the biochemistry of muscular contraction brought to light the extraordinary fact that these two utterly different physiological events involved

some identical biochemical operations; particularly those concerned with a hexose-phosphoric acid ester (13, p. 21). Identical chemical machinery was being used by very different organisms for very different ends.

With his usual perspicacity, Darwin himself had noted the importance of biochemical relationship. In the *Origin of Species* he says:

'Nevertheless all living things have much in common, in their chemical composition, their germinal vesicles, their cellular structure, and their laws of growth and reproduction. We see this even in so trifling a circumstance that the same poison often similarly affects plants and animals, or that the poison secreted by the gall fly produces monstrous growths on the wild rose or oak-tree. Therefore I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed by the Creator' (6, p. 435).

But this suggestion that biochemical similarity implies evolutionary relationship needs some discussion. There are facts that have been interpreted in this way in recent years. The zoologist has good reason for considering that the Chordates and the Echinoderms are more closely related to each other than either phylum is to the Annelids, Arthropods and Molluscs. The occurrence of phosphocreatine only in the glycolytic metabolism of the Chordates and Echinoderms is thus a matter of interest. But by itself this is extremely weak evidence of evolutionary relationship in the way it is sometimes regarded. If we find a sufficient number of biochemical similarities between two kinds of organism we can build up an inductive argument of some strength for their relationship. Thus Fritsch (11) points out that among the algae the Chlorophyceae are related to the higher terrestrial plants by similarity of the various pigments, of their chloroplasts, and of a variety of their metabolic products. A sufficient number of biochemical parallels might relate *Chlamydomonas* more nearly to an oak tree than to the superficially similar *Euglena*. But the strength of the argument depends upon the number.

Often, biochemical similarity cuts right across apparent evolutionary relationship; as in the chemical substances which transmit nervous excitation. The demonstration that in vertebrates nervous transmission across certain synaptic junctions was effected by acetylcholine, was made possible because medical physiologists happened to use as an experimental control an animal with which they had long been familiar, the leech, *Hirudo medicinalis*. By a happy accident the muscles of this animal are highly sensitive to acetylcholine. Thus the remarkable fact emerges that this leech resembles vertebrates in the sensitivity of its muscles to this very peculiar substance. But the resemblance is much greater than that. There is a detailed parallel between *Hirudo* and the Vertebrata in the mode and functional site of action not only of acetylcholine, but also of adrenalin, and of many pharmacologically active substances (30). Indeed such physiological similarities were among the evidence which led Gaskell to put forward his heretical view that the Vertebrates were evolved from that off-shoot of the Annelids, the Arthropods (12). But in spite of all this there is overwhelming evidence that leeches and Vertebrates are utterly sundered in their evolution. We now know that acetylcholine is a common synaptic intermediary at some point or other in the nervous systems of almost all, but not quite all, multicellular animals; though the patterns of pharmacological action are rarely so alike as those of *Hirudo* and the Vertebrates happen to be.[1]

Later another important fact has emerged. Acetylcholine is not found solely where there is some kind of nervous transmission. It occurs even in unicellular organisms, in bacteria and Protozoa, including *Paramoecium* which also forms adrenalin (1). Likewise acetylcholine occurs in plants, where Emmelin and Feldberg (8) have shown it to be primarily responsible for the sting of the stinging nettle.

These peculiar and complex molecules are apt to be found in all sorts of organisms, and special properties which they happen

[1] The injection of curare will paralyse a crab just as well as a frog. But in the former it acts in the central nervous system whilst in the frog it acts at the nerve-muscle junction (17).

to possess are utilised independently in quite unrelated species for some function or other. How far their presence indicates community of descent after the manner Darwin suggests is uncertain. The fact that man and *Paramoecium* both have acetycholine does not necessarily argue for a common ancestry in the same way that the pentadactyl limb does for a man and for a mole. Notwithstanding Owen, it would, for instance, be most surprising if we were to find on another planet an animal with complete structural homology with a vertebrate, including the pentadactyl limb. But, in Owen's phrase, ' the laws of light, as of gravitation, being the same in Jupiter as here ' and the properties of molecules also, if animals did happen to exist on that planet it would not occasion the same surprise to find them possessing acetylcholine and even using it for the transmission of excitation. The possession of a particular kind of molecule, which is known to be widespread amongst very different kinds of organism, might possibly prove to be an inevitable physical necessity for living systems resembling ours. Whereas it seems highly improbable that the unique assemblage of genetic factors which ensures the development of a pentadactyl limb would ever be selected independently on two separate occasions.

This same limitation is seen in the development of respiratory pigments (29). Haemoglobins are repeatedly and independently developed by unrelated animals where there is a functional necessity for an increased power of the blood to carry oxygen. The reason for the abrupt independent evolution of this kind of complicated molecule is not far to seek. Almost all organisms already contain the iron porphyrin compound necessary for its construction, in the intracellular respiratory system of cytochrome. Nor is the haemoglobin molecule solely developed to aid oxygen transport. Keilin & Wang (19) have shown its presence in the root nodules of leguminous plants, where it seems concerned with the process of nitrogen fixation. Often its functional significance is unknown as in the haemoglobin in the brain of some invertebrates, like the minute nemertine *Prostoma graecense*.

Molecules like the haemoglobins are not only easily built up from materials already available in the cell but they have a variety of properties which can be utilised by different organisms for different adaptive ends. Moreover, whilst the essential iron-porphyrin cannot be varied, variation of the protein part of the molecule allows graded variations of physiological properties (9).

The limitation of the sorts of molecule available to organisms for the construction of adaptive systems can be traced directly to the fact that matter is composed of a finite number of elements, each with special properties. Iron and copper have peculiar powers of catalysis, and we find in living cells that both iron-porphyrin-protein compounds and copper-protein compounds are widely utilised as oxidising catalysts (18). Again, the most complex molecules the chemist can construct all involve the peculiar and very varied properties of the non-metals. But there are only some 13 non-metals, and among these the properties of carbon are unique. Biochemistry suggests that organisms make the fullest possible use of the special properties of a very limited supply of suitable elements. Indeed it seems remarkable that the properties of the few kinds of elementary unit available should permit the construction of so improbable a structure as a living organism.

The organism is thus built up of standard parts with unique properties. The older conceptions of evolutionary morphology stressed the graded adaptation of which the organism is capable, just as putty can be moulded to any desired shape. But the matters we have discussed lead us rather to consider the organism as more like a model made from a child's engineering constructional set: a set consisting of standard parts with unique properties, of strips, plates and wheels which can be utilised for various functional objectives, such as cranes and locomotives. Models made from such a set can in certain respects show graded adaptability, when the form of the model depends on a statistically large number of parts. But they also show certain severe limitations dependent on the restricted properties of the standard parts of the set (31). Moreover in this universe of ours any functional problem must be met by one or other of a few possible kinds of

solution. If we want a bridge, it must be a suspension bridge, or a cantilever bridge, and so on. And the engineer who constructs the bridge must choose whichever of these solutions he can best employ with the standard parts at his disposal. In the design of a bridge there are in fact three elements: the classes possible in this universe, the unique properties of the materials available for its construction; and the engineer only takes third place by selecting the class of solution, and by utilising the properties of his materials to achieve the job in hand. He is in a sense merely executing one of a set of blueprints already in abstract existence; though it requires insight to see that the blue-print is there.

We can apply these ideas to the construction of the living organism. Like all material structures they must conform to certain constructional principles. And the blueprints of many of these may be said to be shown to us in that magnificent work of D'Arcy Thompson *On Growth and Form*. The standard parts available for the construction of organisms are the units of matter and energy which can exist only in certain possible configurations. Like the engineer natural selection takes third place by giving reality to one or other of a series of possible structural solutions with the materials available. But the fact remains that we have arrived back at the eighteenth century conception of an ideal plan as an essential constituent of organic design. Yet as L. J. Henderson (14) implies, any such plan is not peculiar to living things; it concerns the whole inanimate and animate universe.

In the child's constructional model of a crane we discern not one principle of design but at least two. For there is the design of the set of parts so that they shall build such things as cranes as well as the special design of the crane. In the living organism we can ascribe the apparent design of its immediately adaptive features to natural selection. Can we discern design in the properties of the units which make such an organism possible? These properties of the units are not the result of selection in the Darwinian sense. And if we see design in them we must say with Du Bois-Reymond '—whoever gives only his little finger to teleology, will inevitably arrive at Paley's discarded "Natural Theology"—' (14). Natural selection bowed Paley and his argument from design out of the front door in 1859; and here he would come climbing in through the back window saying that he owns the title deeds of the whole estate! Fortunately it is for the metaphysicians to examine his claims and not for me.

So far we have confined our discussion only to the molecular level of structure. But once the limitation of standard parts with unique properties is introduced it affects all higher levels as well. For it is a feature of our Universe that combinations of more elementary parts make systems with wholly new properties. In the constructional set, four wheels, two spindles and a plate appropriately arranged will give a truck, with new properties. I would define these as emergent properties; that is, properties which are found in a material structure which cannot be regarded as the sum of the properties of its individual parts. Emergent properties of this kind can be utilised by organisms for all sorts of functions; and an excellent illustration is provided by the uses of fibrillar proteins.

Living organisms can form long chains of carbon atoms. There are not many different ways in which they do this. One comprises the long molecules of fibrous protein (32). By themselves, these long molecules can form flexible threads and membranes. The strength of these structures can be greatly increased by connecting the threads together by means of cross-linkages; just as the long molecules of india rubber may be knit together by sulphur linkages during the toughening process of vulcanisation. That is one of the few ways available for strengthening these structures, and in the Vertebrata this very method of sulphur linkage is employed to make horn, nails and hair. An alternative way of hardening protein is to tan it, just as we do with leather. In this case the long flexible molecules become linked together through the agency of polyphenolic substances. This mechanism has recently been shown to be utilised with marvellous ingenuity by all sorts of animals. The exoskeleton of insects is hardened in this way. So also is the egg-shell of liver flukes. Indeed, wherever

we see a tough brown protein cuticle we have come to expect such a mechanism. And we find it again in the silk of the silkworm, the silk of the stickleback's nest, and in the tough guy-ropes by which the mussel clings to the rocks of the seashore. All sorts of animal independently utilise the emergent properties of this system for different purposes (4).

What is of even greater significance is the fact that such long protein fibres possess the potential property of contractility. The precise molecular change which takes place when such protein threads contract is still under discussion, but one of the most remarkable conclusions of comparative physiology has been that the essential character of muscular contraction seems to remain the same in every kind of organism. The possible ways in which movement mechanisms can be built up by the organism from the available components seems more restricted even than the number of ways of cuticle formation. It is indeed extraordinary to consider that the wing muscles of a minute fly *Forcipomya* which execute a contraction in a thousandth of a second may utilise essentially the same machinery as a lazy sea-anemone, some of whose muscles take six minutes to contract (31, 33). For an engineer to vary speed of movement well over a hundred thousand fold would involve several changes of mechanical principle.

Whilst the property of contractility may be considered as emergent from fibrous organisation of particular kinds of protein molecule, there are other structural characters of contractile systems which we suspect to be emergent in that they appear again and again in unrelated animals, although we are still at a loss to explain their significance. Quite independently in almost every phylum of animals, muscle fibres that develop tension quickly without great change of length are found to be striated. And we see this same phenomenon of striation even in the myonemes of some protozoa, such as *Stentor*. Bernal and others have suggested possible physical analogies to striation. But whatever its physical basis the repeated independent evolution of this complex structure leads one to suppose that it arises from some easily utilised feature of contractile protein threads.

The repeated independent evolution of the complex striation of muscle warns us again that similarity of structural complexity does not always imply relationship between organisms that show it. Unlike the pentadactyl limb, many structures which strike the eye as complex may have a comparatively simple origin through the operation of physical laws on the peculiar properties of matter. Thus ice crystals repeatedly and independently reproduce patterns of great complexity and great beauty ; and the experiments of Leduc on the production of figures resembling mitotic spindles, and of various osmotic growth forms simulating organic structures, only serve to show that our judgment of probable relationship on the grounds of complexity of form may on occasion mislead us (20).

This is particularly the case where we are dealing with microscopic objects and where crystal forces of molecular orientation operate. For this reason we should view with suspicion phylogenetic relationships established on the basis of similarity of cellular secretions, like the nematocysts, however elaborate. It is true that nematocysts may have a very complex structure with an elaborate barbed and spirally decorated thread. But their curious mode of secretion within a vacuole and the high degree of molecular orientation shown under polarised light betrays their semi-crystalline nature. Moreover structures of this sort are formed by organisms some of which must surely be unrelated (15, 23). They occur in the cnidarian Coelenterates, the Cnidosporidia, some Ciliates and some Peridinians ; whilst closely related structures are to be found in some Appendicularians and in the proboscis of Heteronemertine worms. Despite their clear-cut structural features, the chance of independent evolution of these objects may be high, and it is dangerous to build far-reaching systematic arguments upon their characters.

A system at a higher level is the cell itself. We have mentioned the remarkable community of properties in cells from different organisms. There is frequent ability to conduct a wave of electrical depolarisation along the surface. This may remain an incidental phenomenon, as in the cells of the alga *Nitella*. But it may

be utilised independently by all sorts of animals for the conduction of nervous excitation. It may even perhaps be used for the same purpose in the sensitive plants *Dionea* and *Mimosa* (5, 37).

With the building up of cells into special tissues we arrive at a still higher level, that of the organ systems with which the older morphologists were particularly concerned The multiplicity of genetic factors governing these allows of their graded adaptation. Nevertheless, as in lower grades of organisation, the possible kinds of organ system that can be built up from the materials available are severely limited in certain ways. Thus the structure of feeding organs is limited by the special ways in which food is found to be distributed in this universe. For the saprophyte, little is required but a suitable life history that ensures that the surfaces of the body make contact with the external medium. When the food is distributed as a suspension of small particles the case is different. To collect these particles whilst discarding those of no nutritive value entails a change from a random distribution to a highly ordered and localised one ; in physical language there is some decrease of entropy. As in engineering practice, it requires complex and ingenious machines to effect this sort of selective separation ; and the plan of such machines must follow well defined rules. We see exactly these principles emerging in the ciliary feeding mechanisms of aquatic animals. The ingenuity of their devices for the selection of food particles strikes us at once, whilst the rigid mechanical specification to which they must conform forces animals of utterly different evolutionary origin to resort to almost identical mechanical devices. As Orton first showed us, the ciliary feeding mechanisms of Molluscs, Protochordates, Brachiopods and Polychaetes often show points of detailed convergence which almost simulate an evolutionary homology (26).

With animals that feed on large and active prey we also see extraordinary cases of convergence due to the very limited number of possible kinds of machines that can execute the required functions. But in this case the organs concerned are not the same. They are the organs of special sense. It is characteristic of animals, and particularly of these animals, that they show purposive behaviour ; that is behaviour directed towards some future goal. But to do this, they must have some predictor mechanism that guides them to it (38). The data on which prediction is based must be obtained from sensory stimuli, and the amount of information about the probable course of future events is far greater if there is a machine for recording the pattern of stimulation rather than the mere presence or absence of a stimulus. An eye on the principle of a photographic camera can give far more information than a mere light sensitive area.

The possible kinds of stimuli an organism can receive are few and highly specific. Of these, light rays are the most easily employed for the detection of pattern in the external world. But to record the pattern of light stimuli there are only a few possible kinds of morphological mechanism. There is the lenticular eye, which functions like a photographic camera. There are one or two kinds of ommatidial eye. There are also possible instruments based on a process of ' scanning ' the environment with a single receptor in a manner somewhat analogous to the process of scanning used in television. This is essentially what happens in the flagellate *Euglena viridis*. Its pigment-shielded light receptor is pointed in a succession of directions, and the organism steers towards the light by reacting appropriately to the temporal sequence of pattern in the stimulation it receives. It may well be that the development of a scanning mechanism accounts for the remarkable presence of highly developed eyes with lens and pigment cup in some Protista, like the Peridinian *Proterythropsis* (22). Here there can scarcely be any machinery for recording a true retinal image as in eyes with a cellular retina. Many of the kineses and some of the taxes of Fraenkel and Gunn's (10) classification of orientation behaviour can be interpreted as varieties of scanning mechanism ; and such a classification depends largely on the specification of sensory morphology.

Examination of the common features of the organs of special sense in different animals shows us that the demand for patterned sensory information can only

be met by a few highly specific designs from which the animal must choose, if it is to achieve the highly advantageous goal of purposive behaviour. Hence, given the common properties of the limited materials available, inevitably the same kind of complex sense organ must be independently evolved again and again. In this sense, and I believe in this sense only, is Bergson right. There is no need for a vitalistic drive to achieve an eye. But a lenticular camera is an inevitable class of sensory instrument both for animals and engineers.

But the collection of patterned information is only the first stage of predictive behaviour. The use of it requires a computing machine, the brain. Older work on the nervous system focussed attention on the evolutionary homologies of gross structures such as the corpus callosum. Much more attention is now being given to pattern of neurone connexions : the ' wiring diagram.' It has become increasingly evident that the phenomena of behaviour depend in some way on widespread patterns of connexions rather than localised ' centres.' The morphology of such patterns is far more difficult to grasp than the gross morphology of a pentadactyl limb. But such patterns can be immensely rich in morphological structure. Among the very simplest of multicellular animals stand the little Rotifers. *Hydatina senta* contains only 959 cell units in its body. It has a minute central ganglion. But even that wisp of central nervous organisation takes 247 of all the cells in its body (24). In the cerebral cortex of man it has been estimated that there are about ten thousand million cells (25). The patterns of connexions in such systems are much more important than homologous lumps and bumps.

Nothing is more remarkable than the essential similarity of the ' behaviour machines ' which have been independently developed in the brains and sense organs of the most intelligent animals. In particular, recent work (3) enables us to compare the condition in *Octopus* with that in a Vertebrate. In both behaviour is brought about through networks of connexions and pathways of nerves. There is reason to believe that these nerves have essentially similar properties in both groups. In both there are similar behaviour phenomena such as learning and perception of form. But the gross organisation of the Vertebrate and Cephalopod brains shows no homologies. They are entirely independent evolutions from stocks which probably separated before even a central nervous system of any kind had evolved. Not only this, but there have now come to light important parallels between such ' brains ' and the highly complex calculating and computing machines of the electrical engineer. Indeed, similarities have even been suggested between the actual nerve pattern in the visual cortex and the circuit connexions of these machines (38). All who have studied the histology of the nervous system will, I think, agree that such analogies must not be pushed too far. It is nearly as easy to see pictures in these nervous patterns as it is to see them in the fire. But that there is some important degree of similarity in these systems is certain.

The fact is that to achieve purposive behaviour any machine living or non-living must have a mechanism able to integrate present and past stimuli, because these data are needed to predict a suitable course of action. Such machines are necessarily extremely complex and in this Universe of ours there are only certain classes of machine which can do what is wanted. Surprisingly enough more than one stock of animals has independently evolved a nervous machine of precisely this kind, in the teeth of apparent improbability.

All the same, there is danger in supposing that for each functional problem that the animal has to meet there is one and only one possible mechanism ; that there is a unique ' mechanism of behaviour.' As Jennings pointed out long ago, the behaviour of the unicellular Protozoa essentially resembles our own. Yet in this case there are no nerves, no synapses and indeed no complex cellular sense organs. The same end has been achieved by the utilisation of different means. And this same warning is also particularly applicable to insect behaviour. The staggering complexity of behaviour of which von Frisch has shown the honey bee to be capable is all operated through a brain weighing about $2\frac{1}{2}$ mgms. (35) : close on a millionth part of our central nervous

system. As Darwin himself said, 'It is certain that there may be extraordinary mental activity with an extremely small absolute mass of nervous matter: thus the wonderfully diversified instincts, mental powers, and affections of ants are notorious, yet their cerebral ganglia are not so large as the quarter of a small pin's head. Under this point of view, the brain of an ant is one of the most marvellous atoms of matter in the world, perhaps more so than the brain of man' (7, Vol. 1, p. 145). Can we not hear the shade of his old enemy, Samuel Butler, in paraphrase, 'There must have been some cheating somewhere for the *ant* to be so great a winner?' Indeed it seems possible that to achieve so much in so small a volume the Hymenoptera may utilise some cellular property in the construction of their behaviour machines which for some reason is not available to the Vertebrates.

In this address I have tried to show that in the last generation work on the living animal and on its chemistry and physiology has at length brought us back to the central problem of organic design. The principles which emerge are not those which were stressed by the phylogenetic morphologists of the end of the last century. They do not contradict evolutionary principles: they are additional to them. They apply to all grades of structure, from the molecule up to the complex computing network of the central nervous system. We recognise that, as in the case of bridge building, each functional problem before the organism admits of certain possible forms of solution. To meet one or other of these solutions, structural systems can be built by utilising the unique emergent properties of the molecules and higher orders of structural unit which are available. Natural selection will determine which of the various possibilities actually survives. It is therefore not moulding an infinitely plastic organism. It is rather directing it from one possible state to another, rather after the fashion of the moves in a game of chess.

Our conclusion is not unlike that of Owen; that there are two principles governing form. Unlike him, we attribute one of these to natural selection, but like him we recognise in the other an abstract plan. But this plan is not peculiar to living things. There is nothing vitalist about it. It emerges from the unique properties of matter and energy, and even its more complex consequences govern the constitution of inanimate objects like calculating machines, as well as living systems.

This conclusion is far from complete. I have left embryology and many other relevant things wholly undiscussed. It is only the end of a first chapter—and before we go I still want you to notice one thing more. We have been searching for relationships of one kind or another governing form. It is by discerning relationships that science advances. But let us also notice that the faculty by which we do this is aesthetic. The qualities of organic form we seek to understand in *Euplectella* are the very things which impress us with its beauty of design.

And although it is now time for *Euplectella* to be hidden away in the lumber room of the Museum from whence she came, nevertheless before we leave her there, let us reflect upon this; that the very great advance of biology in a hundred years owes not a little to our grandfathers' appreciation of the amazing beauty of the natural world. So if to the dusty lumber room she must go, at least let us set her beside the figure of the Discobolus that Samuel Butler once to his anger found there. Otherwise she may regret that the Linnean rules of nomenclature could not have been made to operate some thirty years earlier, so that at last we could properly be dubbed, Jonathan Swift's type species, *Yahoo sapiens* Lemuel Gulliver, 1726.

References

(1) Bacq, Z. M. (1946), *Biol. Rev.*, **22**, 73.

(2) Bergson, C. 1911, *Creative Evolution*. English trans. by A. Mitchell. London, Macmillan & Co.

(3) Boycott, B. B. and Young, J. Z. 1950, *Soc. Exp. Biol. Symposia*, **4**, 432.

(4) Bradfield, J. R. G. 1950, and Brown, C. H. 1949. Theses for the degree of Ph.D. Cambridge University.

(5) Burdon Sanderson, J. 1888, *Phil. Trans. Roy. Soc.* B., **179**, 417.

(6) Darwin, C. 1859, *The Origin of Species*. Reprint of 2nd ed. 1935 (The Worlds Classics). Oxford, the University Press.

(7) Darwin, C. 1871, *The Descent of Man*, London, John Murray.

(8) Emmelin, N. and Feldberg, W. 1947, *J. Physiol.*, **106**, 440.
(9) Florkin, M. 1944, *L'Evolution Biochimique*. Liege, Editions Desoer.
(10) Fraenkel, G. S. and Gunn, D. L., 1940, *The orientation of animals*. Oxford, Clarendon Press.
(11) Fritsch, F. E. 1935, *The structure and reproduction of the algae*. Vol. 1. Cambridge, University Press.
(12) Gaskell, W. H. 1920, *The involuntary nervous system*. London, Longmans, Green & Co.
(13) Harden, A. 1932, *Alcoholic Fermentation*. 4th Ed. London, Longmans, Green & Co.
(14) Henderson, L. J. 1913, *The Fitness of the Environment*. New York, Macmillan.
(15) Hovasse, R. 1951, *Arch. Zool.*, **87**, 300.
(16) Hoyle, F. 1950, *The Nature of the Universe*. Oxford, Blackwell.
(17) Katz, B. 1936, *J. Physiol.*, **87**, 199.
(18) Keilin, D. and Mann, T. 1944, *Proc. Nutr. Soc.*, **1**, 189.
(19) Keilin, D. and Wang, Y. L. 1945, *Nature*, **155**, 227.
(20) Leduc, S. 1911, *The Mechanism of Life*. London, Rebman.
(21) Mann, T. and Lutwak-Mann, C. 1944, *Ann. Rev. Biochem.*, **13**, 25.
(22) Marshall, S. M. 1925, *Quart. J. micr. Sci.*, **69**, 177.
(23) Martin, C. H. 1914, *Biol. Zbl.*, **34**, 248.
(24) Martini, E. 1912, *Zeit. f. wiss. Zool.*, **102**, 425.
(25) McCulloch, W. S. 1947, *Federation Proceedings*, **6**, 448.
(26) Orton, J. H. 1914, *J. Mar. biol. Ass. U.K.*, **10**, 283.
(27) Owen, R. 1848, *The archetypes and homologies of the Vertebrate skeleton*. London, John van Voorst.
(28) Owen, R. 1849, *On the nature of limbs*. London, John van Voorst.
(29) Pantin, C. F. A. 1932, *J. Linn. Soc.* (*Zool.*), **37**, 705.
(30) Pantin, C. F. A. 1935, *Nature*, **135**, 875.
(31) Pantin, C. F. A. 1950, *Soc. Exp. Biol. Symposia*, **4**, 175.
(32) Picken, L. E. R. 1940, *Biol. Rev.*, **15**, 133.
(33) Pringle, J. W. S. 1949, *J. Physiol.*, **108**, 226.
(34) Raven, C. E. 1942, *John Ray, Naturalist, his life and works*. Cambridge, University Press.
(35) Richards, A. G. and Cutkomp, L. K. 1945, *J. Cell. Comp. Physiol.*, **26**, 57.
(36) Russell, E. S. 1916, *Form and Function*. London, John Murray.
(37) Snow, R. 1924, *Proc. Roy. Soc.* B., **96**, 349.
(38) Wiener, N. 1948, *Cybernetics*. New York, John Wiley & Sons.

THE CENTENARY OF SECTION E (GEOGRAPHY)

Address by

DR. O. J. R. HOWARTH, O.B.E.

PRESIDENT OF THE SECTION

THE significance of this year, 1951, is for us, not merely that it is the centenary of the Great Exhibition, but that it is the centenary of this, the Section of Geography in the British Association. The Section was brought into existence, in circumstances we are presently to recall, at the Ipswich Meeting in 1851. It was the amiable practice of some of its earlier presidents to disclaim their own qualifications for their office. How appropriately I might do the same. But at least, at this centenary meeting, it may be thought suitable that the chair should be a museum piece. I am deeply grateful to be allowed to play that part. And on such an occasion we have a text ready to our hands. ' Let us now praise famous men.'

Of these, the first is Sir Roderick Impey Murchison. A native of Easter Ross in this country, born in 1792, he was educated for the army, and as a young man saw a little service abroad. He did not discover an interest in science until he was in his thirties, but when he did so he lost no time in achieving eminence : he was not out of that decade of his life when he became president of the Geological Society. With his brilliant and in some respects combative career in geology, which culminated in the directorship of the Geological Survey, we are not, however, here concerned.

In one of the immortal Irish stories of the Misses Somerville and Ross it is stated that ' dotted at intervals throughout society are the people endowed with the faculty of " getting up things ". They are dauntless people, filled with the power of driving reluctant spirits before them ; remorseless to the timid, carneying to the stubborn.' Of such was Murchison. In 1830 he played a leading part in the foundation of the Royal Geographical Society ; in 1831, in that of the British Association. He was one of our General Secretaries from 1837 to 1845, and President of the Association in 1846. Nor did he cease, until his death in 1871, to maintain his active interest in the Association generally, and in this Section particularly.

Geography appears in the Association before 1851 as an appendage of geology. As early as 1838 there were papers in Section C on expeditions up the Euphrates, and to Novaya Zemlya and Antarctic waters, and also on surveying. It must be supposed that the eminent geologists who comprised the majority of the sectional committee rebelled against this ; at any rate the name of this early Section thereafter became Geology and Physical Geography, and papers primarily descriptive of travel were for some years neglected. Murchison, possessing as he did some of the better qualifications of showmanship (a useful attribute in General Officers of the Association), found the solution in 1851, when at his instance the Section of Geography and Ethnography was established. (As a matter of history, our letter, E, does not imply seniority over Sections F and G, Economics and Engineering. It had been attached to a Section of Medical Science, afterwards called Physiology until 1847, when that subject merged for some years with Zoology and Botany under the the letter D).

If Mr. E. C. Bentley had flourished a century ago, Section E might have tempted him to modify one of his famous quatrains, and to write that

> The art of Ethnography
> Is different from Geography.
> Geography is about maps,
> But Ethnography is about chaps.

Maps and chaps, however, got along well enough together until 1866, when ethnology, as it had come to be called, turned into anthropology, and harboured in the Section of Biology. We need not here pursue further the history of that Section.

The fact was that Section E at its initiation might without derogation have been called the Travel Section. Exploration was at that time the principal *métier* of British geography, and exploration was news. Therefore Murchison saw the opportunity of creating in the Association a Section which at once would present the knowledge of the earth's surface upon which so much of science depended, and would offer to lay members the interest of seeing and hearing the famous travellers who were busy in extending that knowledge. It is impossible to overrate the personal power of Murchison in the Section from 1851 to 1870. He brought into the chair eminent administrators or army or naval officers whose duties had taken them into distant lands, and if momentarily there was none such to be found, he took the chair himself : he did this no less than seven times in that period.

It was natural, and a well-judged move, that Murchison should bring into the secretariat of the Section a succession of officers of the Royal Geographical Society. The practice continued long after Murchison's time, in fact until 1913. The tenure of office by a recorder or a sectional secretary was formerly not limited, as normally it is now, to five years, and H. W. Bates, author of *The Naturalist on the Amazons*, held one or other of these offices almost without intermission from 1864 to 1881. It must have been much to the Section's loss that he never occupied its chair, probably owing to his rather poor health. Most of the others became presidents of the Section in due course. A somewhat similar relationship with the Royal Scottish Geographical Society came into existence later, and so far as appears these connections in their day were productive of nothing but good.

In early days a sectional president's address was not one of the major items of the programme as it became later, and was not invariably published : it seems in our Section frequently to have taken the form of a few platitudes about the virtues of geography and of exploration, and an indication to the audience of what they were going to hear. In some measure this was regrettable, for the sonorous prose of the early and mid-Victorian period (which seems to have gone out of fashion rather abruptly in the seventies) is worth reading, perhaps in limited doses. Can we conceive of a president to-day charging his audience, as Murchison did in 1863 in these terms : ' In conclusion, I have now only to exhort you to profit by this meeting not merely by listening attentively to the reading of the various memoirs to be brought before you, . . . but also by discussing all doubtful points in a fair spirit of enquiry.' And I may be forgiven for rescuing from the report of 1851 the most exquisite euphemism I have ever encountered : ' Less pains in dressing or clothing children cannot be taken than by the Hindoo : there is no artificial plan of modifying the form in any way.'

It is obviously impossible to attempt any detailed analysis of a century's programmes within an hour. But by way of example, in that first meeting of the Section a hundred years ago the papers ranged widely. The Section was honoured by the presence of the Prince Consort, who communicated a letter written to him by Sir Robert Schomburgk on ethnological researches in Santo Domingo. There were papers on Mexico, on an earthquake in Chile, on Pacific and East Indian islands and in particular on Borneo, on New Zealand, the Himalaya, Asia Minor and Mount Ararat, on the Nile and on Abyssinia, on rapid intercourse between Europe and Asia, and on a canal across the isthmus of Darien. The two last illustrate a type of paper which very naturally was not uncommon in the earlier years of the Section—on potential lines of communication in various parts of the world. In 1852 there was a startling proposal for a waterway right across British North America, from the Atlantic to the Pacific. In 1888 Ferdinand de Lesseps, at the age of 83, presented a paper on the Panama Canal, the French scheme destined in that year to collapse in political strife.

A noble procession of explorers passed across the stage of the Section. David Livingstone was with us in 1857, and the

Section was kept fully informed of his achievements at other times. Indeed Murchison, his close friend, may have exceeded the limit of proper showmanship when he read to the Section Livingstone's affecting letter to him on the death of Mrs. Livingstone. And there was a curious incident in 1867, when Sir Samuel Baker, the discoverer of Albert Nyanza, set out to prove from the chair of the Section that Livingstone must be dead, and pronounced a kind of funeral oration on him ; and Murchison subsequently read a paper arguing that this most eminent of African travellers was alive : Murchison was right, and Baker, despite his first-hand knowledge of Central African conditions, which Murchison did not possess, was wrong. The Section subsequently received H. M. Stanley after his expedition and meeting with Livingstone at Ujiji in 1871. The expedition sponsored by the Royal Geographical Society for the same purpose had failed, and the Society evaded the responsibility of according Stanley the prestige of a meeting in London. The Section stepped into the breach, not without some demur, perhaps understandable, on the part of Stanley.

Livingstone, Baker and Stanley had all been inspired by the journey made by Richard Francis Burton and John Hanning Speke when in 1858 they discovered Lake Tanganyika, and Speke, pushing forward when Burton was disabled, found Victoria Nyanza and believed it to be the source of the Nile. It will be remembered that Burton disputed this, that in the course of a later journey Speke and James Grant proved the point to demonstration only short of actually tracing the course of the river below the lake, but that Burton still disbelieved. Therefore in 1864 a debate between the two men was arranged in the Section, which might have achieved an equal heat with that between Hooker and Huxley and Wilberforce, Bishop of Chichester, on the Darwinian theory of evolution, in Section D in 1860. But Speke was killed in a shooting accident the day before the discussion was due, and Burton very properly substituted communications on his travels in West Africa. At a later date (1878) he addressed the Section on the land of Midian.

Alfred Russel Wallace was with us for the first time in 1863, and it is worth remark that he appeared in this Section as an ethnologist, lecturing on Malays and Papuans. His papers on the physical geography of the Malay Archipelago and on the geographical distribution of animal life in it were given in Section D, when he traced the Wallace line beloved later of scholastic geographers. There was of course perfectly good reason for the second of these papers being presented to that Section ; but as for the other, it may be wondered whether he shared the suspicion of our own Section which undoubtedly was felt by some most eminent scientists at that time—the suspicion that geography, as Murchison conceived it, was not a truly scientific subject. For Murchison himself, as a geologist, was at issue with such masters as Lyell and Darwin : he was a catastrophist, and he did not accept the theory of evolution. Even Darwin, gentlest of men and of critics, wrote to Lyell after the publication of *The Origin of Species* : 'I expected Murchison to be outrageous. How little he could ever have grappled with the subject of denudation ! How singular so great a geologist should have so unphilosophical a mind !'

This, however, is to diverge from the topic of travel, to which, indeed, we must not devote much more time. There were many other travellers who appeared in their fame before the Section—Sturt, half blinded after his last expedition into central Australia ; the Schlagintweits from the Himalaya ; Fitzroy the commander of Darwin's ship the *Beagle* ; Palgrave from Arabia, 'the spirited and eloquent Palgrave,' as Murchison described him, 'the first intelligent person to traverse that country to the Persian Gulf' — Murchison had a striking command of epithets—Scoresby, disproving in 1853 the popular conception of an 'open polar sea,' on the ground that, with regard to the loose ice seen on the confines of the Arctic region, 'the more probable inference is, that these ices are merely the excess of the winter production, by the severe frost of the north, carried off under the wise and gracious œconomy ordained by the Creator' ; Clements Markham, appearing on the platform of the Section for the first time in 1857 (in his 27th year) to discuss the search for Franklin's

expedition ; Edward Whymper, fresh, not from the Alps, but from Greenland ; Sir Erasmus Ommaney who brought to the Chair of the Section in 1877 a general account of Arctic travel—and a host besides which cannot be catalogued here. In later years our attitude towards explorers as such was modified : I recall, for instance, no special endeavour to secure the presence of Peary or Scott or Shackleton by way of signalising their personal achievements. Not that the Section ever failed to take note of exploration : as an illustration, those of us who were in Australia in 1914 do not forget the joint meeting of Sections C, D, E and K, with William Bateson, President of the Association, in the chair, when the subject was Past and Present Relations of Antarctica in their Biological, Geographical, and Geological Aspects. Sir Douglas Mawson opened ; Griffith Taylor, Edgworth David, Albrecht Penck, Sir George Simpson and Sir Albert Seward were among those who took part. (Penck, by the way, was at that time an enemy subject : the first German war had broken out as we reached Australia, but the Australian authorities tolerantly allowed the distinguished German scientists who had come with us to participate in the Meeting. The international bonds of science were not easily loosened in those days).

Turning from travel to other departments of geography, we find that in the earlier years of the Section's life there may have been some justification for the mistrust, already mentioned, with which some scientists regarded the subject as a science. Thus in 1857 we received a peculiar paper which sought to prove that the Gulf Stream and the drift following it had nothing to do with the relatively warm winter climate of the British Isles, but that this was due to vapours. In 1858 another paper, on the configuration of the surface of the earth, indicated that ' the currents of a primitive ocean, as determined by known physical laws and modified by the mountain systems already formed . . . necessarily . . . led to the formation of the great continents in their present position, and when taken in connection with the vertical forces operating from beneath, . . . account for all their geographical and geological features so far as they have hitherto been investigated.' Perhaps you can work out the train of thought in that sentence : it is beyond me.

Teething troubles of that sort were soon over, but it was far otherwise with the notorious disputation on the question ' what is geography ?,' In the days when this was most rampant, I for one found no difficulty in losing patience with it, though in retrospect it is not without interest. There are many departments of science, outside the geographical field, between which there are certainly frontier regions, but as certainly not rigid boundary lines. Of course this has been said before (there is nothing in this address which has not), notably by Leonard Darwin in his address in 1896, at the time when he was a secretary of the Royal Geographical Society. ' My own humble advice to geographers,' he said, ' would be to spend less time in considering what geography is and what it is not ; to attack every useful and interesting problem that presents itself for solution ; to take every help we can get from every quarter in arriving at our conclusions ; and to let the name that our work goes by take care of itself.' His advice was not immediately followed. The attempt to lay down boundary lines around geography was wasted effort ; but one saw why it happened—I can illustrate this from my own recollection by taking as an example one of our leaders, H. J. Mackinder. It was said of Mackinder by historians that he knew no history, by geologists that he knew no geology, by climatologists that he knew no climatology. That was untrue : he knew, in those subjects those things which his geographical purpose required. However, the heart-searchings as to the scope of geography were born of that mistrust of the subject as a department of science to which reference has been made already. This mistrust was countered by exaggerated claims made by its protagonists, some of whom went near to asserting that geography was the foundation of all science. Sir Samuel Baker, for instance, told us in 1867 that geography was worthy of the ' high position ' of a Section in the Association, ' as nearly every science is dependent on our knowledge of the earth ' : he went on to associate the subject with theology and the Creation, and subsequently

traced the influence of the geographical outlook in human affairs to the point when it became, as he said, ' the base of diplomacy,' and its study the necessary prelude to such objects as the provision of ' a new and direct route to India,' since ' without yielding to exaggerated alarm, we must watch with intense attention the advances of Russia upon the Indian frontier.'

The scope of geography was not, so far as appears, formally debated in the Section, but various presidents placed various versions before it, in manners not always acceptable to the stricter scientists in contiguous Sections, nor, indeed, equally to all geographers. Among the earlier addresses, that which revealed by far the clearest conception of the subject as it is now understood was given in 1875 by Lt.-Gen. Richard Strachey (afterwards Sir Richard Strachey), of the Royal Engineers, who was already a Fellow of the Royal Society, and was to become President of the Royal Geographical Society (1888–90). He was a man of immense erudition and wide interests : in the course of his public service, rendered mainly in India, he was concerned especially with irrigation, forestry and finance, and he added materially to knowledge of the geomorphology, geology and botany of the Himalaya. As he was not a professional geographer, there is some risk that he may be forgotten as a pioneer of this century's progress in geography, and that would be an injustice. In his address to us he acknowledged that his predecessors in the chair had ' viewed geography in many various lights,' but he himself claimed to follow ' a somewhat different path from those who have gone before me in treating of geography,' and he proposed ' to speak of the physical causes which have impressed on our planet the present outlines and forms of its surface, have brought about its present conditions of climate, and have led to the development and distribution of the living beings found upon it.' He drew a distinction between the popular conception of geography which stopped at observations and description, and what he called physical or scientific geography, which involved the classification and comparison of the facts so accumulated.

Strachey's work as a leader in geography was widely appreciated ; in our own Section by Sir Joseph Hooker, and in later years by J. S. Keltie. Hooker was President of Section E at the Jubilee Meeting of the Association, in 1881, when all the sectional chairs except one were filled by past Presidents of the Association. Of Strachey he said that a lecture given, not to us, but to the Royal Geographical Society in 1877, contained the ablest review of the subject known to him. ' It might very well,' said Hooker, ' be entitled The Whole Duty of the Geographer. Every traveller's outfit should include a copy of it, and one should accompany every prize given by the Geographical Society to students for proficiency in geographical knowledge.' Hooker came before the Section as a botanist interested in distributions : apart from a casual reference or two in earlier years, he actually introduced the work of Humboldt to the Section, and he presented Darwin, de Candolle and Alfred Russel Wallace in their rôles as geographers. This was an address of a rare type.

Clements Markham's address to the Section in 1879 traced the progress of geography from exploration through map-making to the study of land-forms and distributions, the air, and the sea, and to the effects of natural environment on man. Then—and this was the unusual feature of his address—he presented a geographical study of the Don valley, the meeting being in Sheffield. Being himself a visitor, he apologised to the local residents among his audience for introducing this subject to them, but it probably enlarged the outlook of most of them to hear him applying his own principles to the study of their home territory.

E. G. Ravenstein in 1891 thought the time had come again to survey the field of geography, and in doing so said nothing new, though he was explicit on the point that the geographer should know the foundations of his subject, and also should know—really know—one or more of the departments erected on that foundation. And that was wise advice, for this was preceisely what other scientists in contact with geographers said that geographers did not do, and no doubt in those days there was some ground for the accusation.

Ravenstein himself could not be so assailed, for no one could gainsay his profound knowledge of the history of cartography, the subject which occupied most of his address; moreover, as he pointed out, whatever subjects might be argued about as within or without the scope of geography, cartography was not one of them.

Mackinder in 1895 and H. R. Mill in 1901, in presidential addresses to our Section, both adopted the historical approach to discussions of the scope of geography. Mackinder was speaking in the year in which the International Geographical Congress had been held in Britain for the first time, and he took occasion to lay before the Section a conspectus of the work of Humboldt, Ritter, Petermann, Berghaus, Peschel, and others in Germany. If it occurred to any of his audience to do so, they might have wondered at the time-lag between the foundations of these Germans' work and their appreciation in this country. Yet we must not too loudly decry British geography of the nineteenth century. Mackinder put it characteristically and forcibly: 'Speaking generally, and apart from exceptions, we have had in England good observers, poor cartographers, and teachers perhaps a shade worse than cartographers. As a result, no small part of the raw material of geography is English, while the expression and interpretation are German.' In this stricture you notice that he said 'England' and 'English': we may prefer to believe that he did not in these terms include 'Scotland' and 'Scottish.'

Mill approached the subject rather as a research worker than as a teacher, and that is what might be expected from the differences of outlook between him and Mackinder. Mill, first, I believe, among those who had addressed the Section on the subject, set forth his own definition in an italicised sentence: 'Geography is the science which deals with the forms of relief of the earth's crust, and with the influence which these forms exercise on the distribution of all other phenomena.' There the italicised sentence ends, you may think too soon; but the text continues: 'This definition looks to the form and composition of the earth's crust itself, and to the successive coverings, partial and complete, in which the stony globe is wrapped. We sometimes hear of the New Geography, but I think it is more profitable to consider the present position of Geography as the outcome of the thought and labours of an unbroken chain of workers continuously modified by the growth of knowledge, yet old in aim, old even in the expression of many of the ideas that we are apt to consider the most modern.' If we are tempted to wonder whether Mill's view of the scope of geography differed materially from that expressed by Strachey twenty-six years earlier, that need imply no criticism of Mill; and Mill himself cast back to a predecessor nearly three centuries earlier than he. In the course of a review of what he termed 'Some Historical Landmarks,' ranging from Ptolemy to Kant, he introduced the name of Nathanael Carpenter, of Exeter College, Oxford, as the first English scientific geographer, contemporary of Varenius and Cluver. Carpenter published his *Geographie* in 1625, and in it he dealt with mathematical geography and cartography, continents and islands, mountains, valleys, forests, rivers and lakes and seas, climates, and the influences of geographical environment upon man. Modern geography, forsooth. No doubt with reason, Mill expressed the fear that most of his audience had never heard of Nathanael Carpenter.

Sir Thomas Holdich in 1902 viewed geography, as others have, as a foundation subject, saying that 'a collector of facts like myself finds it convenient to accept, for the mere sake of simplicity, the science of geography as the best basis for divergent inquiries into many other scientific fields, which can be differentiated at leisure by the natural philosopher.' And indeed there have been distinguished geographers who have done just that: the cousins Leonard Darwin and Sir Francis Galton are outstanding examples, whose names remain no longer in general estimation as geographers, but as anthropologists in a special field. And Mackinder passed on from geography to statecraft, though never neglectful of his primary interest.

We need not further pursue the subject of the scope of geography through addresses by subsequent presidents. Some of them referred to the question incident-

ally ; but the earlier disputes were dying down. In the eleventh edition of the *Encyclopaedia Britannica*, which appeared in 1910, Mill modified his definition of geography in these terms : ' the exact and organized knowledge of the distribution of phenomena on the surface of the earth,' which improves upon his wording of ten years earlier. If anyone had presented Mill's definition of geography sixty years before, we should have heard of people countering it with some such variation as the inexact and disorganized knowledge of the distribution of some of the phenomena on bits of the surface of the earth, or words to that effect. Geography may well be a primeval science ; Adam and Eve may well have been the first geographers as well as the first pomologists and herpetologists. What geography has achieved in the century we are reviewing, and what Section E has very effectively aided in achieving, is to demonstrate an outlook wider in horizon than those of the other departments of science with which it is associated, and to help to bring them into a harmonious focus. It has been asserted that geography is no more than a point of view. So, if you come to that, is all science a point of view, and one of the most exalted to which man has attained.

Sir Charles Arden-Close, addressing us in 1911, showed that out of original contributions to the proceedings of the Royal Geographical Society during the five years 1906 to 1910, explorations still accounted for 57 per cent., physical geography in all departments (including land forms, seismology, vulcanology, glaciology, hydrography, oceanography, meteorology and terrestrial magnetism) for 29 per cent., economic and social geography for 7 per cent., mathematical and cartographical geography for 3 per cent., the few remaining communications being anthropological and biological. Our own distribution of papers in those same years was different. Our papers on exploration were only 22 per cent. of the whole, on aspects of physical geography 43 per cent., on economic and social geography (taking that division to include geography in education) 23 per cent., and mathematical and cartographical geography 12 per cent. Anthropological and biological subjects are of course covered in other Sections of the Association. These figures suggest a cleavage between the policies of the Society and the Section ; indeed, there was about that time such a cleavage, though it merely eased, not forcibly disrupted, the close relationship between the two organisations which has been mentioned already. We should glance at some of the main trends among the subjects referred to.

The Section was informed from time to time as to the progress of survey methods, but on the lack of progress in surveying it was from time to time critical. Such lack was not the fault of geographers but of administrators. So we find Murchison in 1863 complaining of the incompleteness of the Ordnance Survey, a defect in due course remedied. In 1872 Galton devoted most of a brief address to means for making Ordnance Survey maps more easily accessible to the public, and his proposals bore fruit. At intervals through the century there are reflections on the need for surveys in overseas teritories of the Empire. Mill commented on this in 1901 ; Reeves in his address in 1916 on the mapping of the world indicated the position clearly, and it is one upon which the Section did, and does, well to keep watch, for it is always possible, if the Council sees fit, to put the weight of the British Association behind a recommendation.

As an incident in our Section's history, the Palestine Exploration Fund was a cause to which the Section gave a good deal of attention in 1866–75, maintaining a collaborating committee, through which contributions to the fund were made by the Association. And we may remind ourselves of papers in 1877 and 1878, on levels from the Mediterranean Sea to the Sea of Galilee, and on a survey of Galilee respectively, by Lieut. H. H. Kitchener, R.E., a young man destined for the highest rank in his profession. We should recall also, with gratitude, Mill's conception, brought before us in 1896, of a geographical description of the British Isles based upon sheets of the Ordnance Survey, of which he himself provided an example, now in the fullness of time to be followed on a comprehensive scale. Nor, I hope, was the Section unhelpful toward that outstanding geographical undertaking, the Land Utilization Survey.

In 1857 William Hughes, a man whose pioneer activities in geographical education may be in some danger of oblivion, entered a plea for decimal scales in all maps, and particularly in atlas maps, the scales of which, he said, though uniformity is unattainable, should always bear a decimal ratio one to another. He must have been something of a pioneer in pointing out that, for educational uses especially, ' proportionate distances and magnitudes, referred to an easily comprehended standard, are the basis of all sound geographical knowledge.' One might have supposed that Hughes' lesson would have come home to cartographers by now, but this is not always the case. Only last year the lament of an eminent geographer was heard in a published letter concerning a flight to Montreal, in which these words occur : ' I did not think it could be so far along the river to Montreal,' and later, ' No school atlas ever gave a child any idea of the size even of Montreal Island. Damn atlases.' There is much to be said for this view. Yet in Edinburgh such a charge, or any charge against British cartography, will be received with equanimity, for here has been achieved a standard of scientific cartography unsurpassed elsewhere in the world.

Ravenstein's address on the history of maps we have noticed already. In 1892 he brought before the Section Penck's conception of a world map on the 1/M scale. Herbertson in 1896 spoke to the Section on the meteorological atlas. Among later communications, we must not fail to take note of the address in 1925 by A. R. Hinks, who brought to bear on cartography not only a profound knowledge of the mathematics of the subject, but also a distinguished sense of the artistic merit in maps which a few cartographers do, and all should, strive to achieve. A sectional committee in 1912–13 studied the subject of atlases and maps for school and university use, not, I believe, without effect.

Geography, as Mill and others have said, is a tree of age and long growth, but looking back over our century we certainly find a new strength, a richer foliage, beginning to appear in the late seventies and the last two decades of the nineteenth century. In our own Section, one gets the sense that Strachey in 1875 and Markham in 1879, in colloquial phrase, started something. In physical geography the treatment of subjects began to take on a new precision. Godwin-Austen gave us an address on the Himalaya in 1883 which marks a forward step in the geographer's view of a mountain-system ; and James Geikie's address in 1892 on coast lines was unlike any before it in bringing a definite code of study to bear on a particular class of physical features. In 1893 a full-dress debate was staged between geographers and geologists on the limits between physical geography and geology, Clements Markham leading. A more indeterminate discussion could scarcely be conceived, and Sir Archibald Geikie, from the chair, summed up to the effect that a hard and fast boundary line between the two subjects was neither necessary nor desirable.

In 1894 Penck published his *Morphologie der Erdoberfläche*, and in 1897 one suspects that the Section missed an opportunity when Penck, W. M. Davis, and Kropotkin were present : one can so well conceive these three meeting in some broad morphological discussion ; but their communications lay apart. Kropotkin spoke of the direction of lines of structure in Eurasia, Penck of the need for the study of rivers, Davis merely of geography in universities. The subject of geomorphology now claimed increasingly wide attention : I remember Herbertson speaking to me of ' this whole great new science,' a phase which has remained in memory probably because at the time (it must have been in 1901) I did not know what geomorphology meant. It is well known that Herbertson's conception of natural regions, through which he became and will remain one of the outstanding figures in the history of British geography, was rather coolly received when he first introduced it to the Royal Geographical Society in 1904, and the regions did not appear in our Section until 1913, when he opened a discussion on them. Meanwhile, he had been our president, in 1910, and it is perhaps significant that he then referred only incidentally to natural units, not regions. But his fine conception was to prevail.

In 1915 Sir Henry Lyons was calling the

Section to more and more research in the science of geography—he stressed the science—in all departments, but especially the physical. Mackinder in 1931 wished to regard the hydrosphere, not the lithosphere, as the foundation for geographical research, and he relegated geomorphology to the position of a marginal subject, an interesting argument which I confess that I was then, and still am, unable wholly to follow. However, inasmuch as in later life Mackinder was taking that view, it is of some interest to recall that Sir John Murray in 1899 opened his presidential address by soundly rating the occupant of the chair in 1895, who was Mackinder though Murray did not name him, for making no mention of oceanography in the 'very able and detailed review' of the progress of geography down to that year and since the foundation of the Association. Murray, as a participant in the voyage of the *Challenger* from 1872, and editor of the scientific reports of that voyage which were completed in 1896, naturally devoted his address to oceanography : he dated from this voyage the recognition of the subject as a distinct branch of science, and properly so, though the Section had not failed in earlier years to take note of the study of the sea, and has continued to give place to it in its programmes, including that of the present year.

At quite an early stage the Section was introduced to that unblessed word anthropogeography, and a general conception of the influences of environment on man was indicated by various speakers to the Section almost from its beginning. In view of our co-operation with Ethnology this was natural, and there is a strong suggestion that that co-operation helped to foster the progress of what came to be known as human geography.

Herbertson planned a work on human geography ; indeed, I saw fragmentary proofs of the beginning of it, heavily corrected, as death compelled him to leave it. Dr. Marion Newbigin in 1922, and one of Herbertson's most distinguished pupils, P. M. Roxby, in 1930, gave addresses on human geography to this Section, which, taken together, provide directives of thought in this subject which, I feel sure, are less accessible than they should be, in having suffered interment in the annual volumes which the Association used then to publish. I think Roxby was the first to present to the Section, at least with any measure of analytical judgment, a view of the work of the French geographers. A part, however, rather than the whole of this department came first under particular notice during the fruitful period at the end of the nineteenth century. This was what was known at that time as commercial geography, which first appeared in our programme in 1884 in a paper 'On the British Commercial Geographical Society, about to be founded.' The subsequent history of this proposal is unknown to me. In 1889 and 1890 the subject was taking good hold. Sir Francis de Winton in the former year set out its elements in his address, as if it were something new ; Mill spoke on the physical basis of commercial geography ; in the following year Keltie took up the commercial geography of Africa, and there were other papers on the subject. By 1907 George Chisholm had long been recognised as a leading authority on commercial geography : his address to us in that year took the form of a history of certain trade routes. Dickson's in 1913 reviewed existing routes on the broadest basis, and he concluded with a plea for a stock-taking of natural products, which, he said, while drawing upon the knowledge amassed by science in other departments should be the task of no one or group of these, but of geography. Dickson, late in his life, became an encyclopaedist : how ably, had the opportunity come to him, would he have conducted such an inquiry as he had in mind.

A first contact with the possibility of geography as an educational subject remains in mind because it coincided with my first acquaintance with Sir Clements Markham. The year was 1896, my last at Westminster School, of which Markham was a devoted *alumnus*. It was a hallowed practice among Old Westminsters, on attaining high honours, to appear at the School and invite the head-master to grant a 'late play,' which being translated signifies an additional half-holiday. Markham did this, and spoke to the School about geography, and then he advised us all to specialise in something. 'Look at

me,' he said, 'I have specialised in geography, and here I am a K.C.B.' I believe we thought him a rather conceited old man (which he was not), and we communed among ourselves how anyone could conceivably specialise in geography, since for us it had stopped short at the questions and answers in the primers of extreme childhood. The movement toward improved geographical teaching in schools had not then reached Westminster. This had been initiated in 1869 by the Royal Geographical Society under the inspiration of Francis Galton, who did not refer to this matter in his address to the Section in 1872, though the Section received in the same year an account of geography teaching at Eton, where it seemed to be carried on with good judgment. Otherwise the Section was not used as a sounding-board for the scheme, which was abandoned in 1884 since only very few schools remained in support of it.

It is well known that the Society was not thereupon discouraged, but in the same year appointed J. S. Keltie as its inspector of geographical education, and charged him with the investigation of the subject in other countries. He produced a report, and an exhibit of foreign text-books and appliances, to which the cliché may truly be applied that in Britain they marked the beginning of a new epoch. The Society thenceforward concerned itself less with school-teaching and more with the status of geography in universities; first in Oxford and Cambridge, then elsewhere, until in 1930 Mill was able to write that 'there is now a geographical department in every British University where forty years ago there was none.'

That, then, is one of the major achievements in geography within our century—its establishment as a subject of instruction in schools and universities. Looking back, one sees this as the result of a magnificent piece of team work. It seems now a remarkable feature that those who sponsored the movement should have convinced the educational institutions that they were right, while they themselves, or some of them, were still having to argue about the definition and scope of their subject. Whatever obstacles may have been encountered at the time, the movement, viewed historically, grew like a snowball. Besides the Royal Geographical Society, the Royal Scottish Geographical Society and many other societies fostered it. Its effect on the programmes of our Section was strongly marked. While the Section had touched little upon education down to 1885, in that and subsequent years there was a whole spate of addresses and papers on the subject. Keltie began it; Goldsmid, Freshfield and Mill followed in 1886; Sir Charles Warren devoted his address to it in 1887. Among many wise observations he reminded the Section of the value of geographical knowledge 'more particularly' of 'our own Empire'—an aspect of the matter which some teachers, at any rate, preferred to ignore. To persons ultra-nationally minded the word 'empire' may indeed have been anathema and the conception of a commonwealth repulsive; but in later years that trend of thought has without doubt diminished. Warren's conception of geographical rhymes for young children, set to music and sung, indicates at once the breadth and the youthfulness of his mind: it conjures up (though he did not say so) the delicious imaginary picture of a committee of the Section charged with composing such aids to memory authoritatively, and by way of report, performing them in concert in the Section-room.

In the same year (1887) Mackinder promulgated a scheme for geography at the universities; Keltie reviewed the situation again in 1891; H. N. Dickson in 1892 stressed the need for the teaching of meteorology at a time when, he said, there was no such teaching anywhere in the country (he and others were to remedy that not many years later); in 1893 B. B. Dickinson outlined the scheme of an association for the promotion of geographical teaching which had been put forward at Oxford earlier in the year, with what fertile results, under its honoured name of the Geographical Association, we all know. In later years Mill, Douglas Freshfield, Herbertson, David Hogarth and Sir John Myres were among those who made the most important communications the Section has received on geography in education. In 1903, and again in 1929, we discussed the matter jointly with the Educational Science Section (L),

Mackinder leading on the first, Myres on the second occasion. One of the Section's research committees in 1923 and subsequent years made a survey of geographical teaching which should by no means be overlooked today. A practical demonstration we must not neglect here in Edinburgh is Professor Patrick Geddes' Outlook Tower, which he described to us at the meeting in 1895.

Nor must I neglect a dutiful tribute to those who taught me the most of what geography I knew. The Oxford trio of Mackinder, Herbertson and Dickson was an exceedingly powerful combination. Mackinder by his oratorical gifts alone would have focussed attention upon any school or cause. Of geography, as presented by Mackinder, it might be said as Wordsworth wrote of Milton :

> The Thing became a trumpet ; whence he blew
> Soul-animating strains,

and I recall him as more effective in the lecture-room than in the laboratory. The reverse was true (to me at any rate) of Herbertson. On the platform his enthusiasm was in a measure concealed by a rather flat delivery ; as a tutor, he was infinitely patient and admirably lucid. As for Henry Newton Dickson, if I am biassed by the memory of a close friendship, I make no apology. He left not much in writing, but I believe his mind to have been more scholarly than Mackinder's, more agile than Herbertson's. He made original contributions to the study both of the weather and of the sea ; he was a good mathematician and a trained surveyor. Later, when he left Oxford for Reading, the scope of the chair he occupied there directed his attention to economic geography : this, as we have seen, was the subject of his address to Section E as president in 1913. As a chief Dickson was wholly admirable, as none knows better than I who served under him in the Geographical Department, Naval Intelligence, from 1915 to 1919.

Time presses, and I have omitted so much. I have said almost nothing of the research committees from time to time appointed on the recommendation of the Section, whose merits deserve a review to themselves. I have indicated little of the real range of the geographical outlook. I have refrained from trespassing on the land of the living : that I should comment upon the work of our present geographers would have been merely impertinent. What I have tried to show by illustrations from the work of this Section, is that in the century which is past British geographers have enunciated their principles and begun to apply them ; have indeed applied them so far that the time is ripe, I believe, to express them, not only in terms of individual researches (as is done) but in terms of a new geographical encyclopaedia. That is easily said, less easily done against the barrier of present-day costs. Yet some of us, I trust, may see it done. Meanwhile may I touch in a few words on one more of the many facets of geography.

I made, a while ago, a flippant reference to Adam and Eve. I apologise to their shades, for they must have loved their Garden of Eden. Such earth-love must always have been a primitive, deep-seated instinct of man. We are, if you care to coin a word, geophilists. Of this love are born farmers and travellers and geographers, including such enthusiasts as the advertiser in *The Times* not long ago, the ' six-foot geographer (female), ' who asked for an ' adventurous post.' But in some this love of the land is absent or latent because of the setting of their lives in places where the beauties of the land are not seen. A speaker to the Association for the Preservation of Rural Scotland was quoted recently as saying that not enough emphasis was placed in the schools on where men came from—the soil. Somehow, he said, they wanted to get at the children at school so that it would be a joy for them to go into the country and see beatiful things. That is obviously a call to geography : it is a corollary to T. H. Huxley's charge to ' seek the meaning of the phenomena offered by the brook which runs through our village, or by the gravel pit whence our roads are mended.' To teach the beauties of the land for the sake of their preservation must surely be a high function of geography. It may be conceded that a greater proportion of us is sensitive to those beauties than was the case a century ago, and alongside the organisations and individual activities

which work for their preservation I like to believe that the teaching of geography has had material effect.

In this connection we owe constant attention to the literary side of our subject, for much can be achieved by well chosen words, whether with or without opportunity for visual guidance. Douglas Freshfield saw that, in his address to us in 1904 on the mountain scenery which he loved. Vaughan Cornish saw it and expressed it in his own earnest if rather angular style. Mackinder saw it in his first address when he spoke of geomorphology as the half-artistic, half-genetic consideration of the form of the lithosphere ; and again, in his second address, when he said that he 'would have the young geographer practised in the use of an almost Ruskinian, purely descriptive language.'

We should avoid by all means the too facile use of common epithets, such as occurred in a text-book I had to read years ago, in which that grievously overworked adjective 'great' occurred nine times on one page (and I dare say nine hundred times in the whole book), applied impartially to the works of man and the wonders of nature. We have guidance ready to hand in the works of some of the more eminent travellers, and also in those of certain writers of fiction. I have no pretension to be thought well read, but among the second of these classes my best beloved writers are two, fitly to be named here in Scotland—Robert Louis Stevenson and John Buchan. Each of these had an outstanding gift of presenting incidents in exquisite settings of scenery and even of atmospheric conditions. May I give one example—Stevenson's description of an atoll in the Pacific Ocean from his novel *The Ebb Tide* : 'The isle—the undiscovered, the scarce believed in—now lay before them and close aboard ; and Herrick thought that never in his dreams had he beheld anything more strange and delicate. The beach was excellently white, the continuous barrier of trees inimitably green ; the land perhaps ten feet high, the trees thirty more. Every here and there, as the schooner coasted northward, the wood was intermitted ; and he could see clear over the inconsiderable strip of land (as a man looks over a wall) to the lagoon within—and clear over that again to where the far side of the atoll prolonged its pencilling of trees against the morning sky. He tortured himself to find analogies. The isle was like the rim of a great vessel sunken in the waters ; it was like the embankment of an annular railway grown upon with wood : so slender it seemed amidst the outrageous breakers, so frail and pretty, he would scarce have wondered to see it sink and disappear without a sound, and the waves close smoothly over its descent.' If ever we torture ourselves to find analogies, or aspire to frame descriptions in such transcendent terms, at least we shall be making a worthy endeavour.

Have you noticed the high proportion of Scotsmen who have set their names upon the roll of progress in this past century of geography—Murchison, Livingstone, the Geikies, Keltie, Mill, Chisholm, Herbertson, Dickson, Geddes, and many more than these? I am not trying to produce a calculated effect for the occasion of a meeting in Scotland. Whence, then, came their inspiration? If I have guessed rightly, it must have been a gift of grace from their own most lovely land.

APPENDIX

Orationis Personae

H.R.H. ALBERT Francis Charles Augustus Emmanuel, *Prince Consort* (1819–61), F.R.S., son of Ernest, Duke of Saxe-Coburg-Gotha ; married Queen Victoria, 1840. President, B.A., 1859.

BAKER, Sir Samuel White (1821–93), F.R.S., explorer of Nile headwaters and tributaries, 1861–1865 ; governor-general, Upper Nile region, 1869–73.

BATES, Henry Walter (1825–92), naturalist, explored Amazon, at first with A. R. Wallace, 1848–59 ; published *The Naturalist on the Amazons*, 1863 ; assistant secretary, Royal Geographical Soc. 1864–92.

BERGHAUS, Heinrich (1797–1884), Prussian trigonometrical survey, 1816 ; geographical school at Potsdam ; compiled *Physikalischer Atlas* (Gotha, 1838–48) and other atlases ; works on general and German geography.

BURTON, Sir Richard Francis (1821–90), explorations in Arabia, 1853 ; Somaliland, 1854 ; Nile sources, 1856–59, etc.; K.C.M.G., 1885.

CANDOLLE, Augustin Pyrame de (1778–1841), Swiss botanist, was a colleague of Cuvier and Lamarck, and set forth a principle of botanical classification according to natural methods. His work was carried on by his son Alphonse and grandson Anne Casimir.

CARPENTER, Nathanael (1589–c. 1628), schoolmaster in Dublin ; fellow of Exeter College, Oxford,

1607 *seq.*; D.D., 1626 ; author of geographical and philosophical works.

CHISHOLM, George Goudie (1850–1930), published *Handbook of Commercial Geography*, 1889 (10th ed., the last in his lifetime, 1925) ; lecturer in geography, Edinburgh, 1908–21 ; reader, *ib.*, 1921–23 ; Sec., R. Scottish Geog. Soc., 1910–25.

CORNISH, Vaughan (1863–1948), director of technical education, Hants., 1890–95 ; subsequently travelled extensively ; studied wave formations in sea, rivers and sand ; also the appreciation of natural scenery, preservation, etc.

DARWIN, Charles Robert (1809–82), F.R.S., naturalist on H.M.S. *Beagle* during voyage round the world, 1831–36 ; began notes on transmutation of species, 1837 ; published *Origin of Species*, 1859.

DARWIN, Major Leonard (1850–1943), R.E., 1871–90 ; observations for longitude in Australia ; also solar eclipse, Grenada, 1888 ; president, R.G.S. 1908–10 ; student of bimetallism ; president, Eugenics Soc.

DAVID, Lt.-Col. Sir Tannatt William Edgworth (1858–1934), C.M.G., 1910 ; K.B.E., 1920 ; professor of geology, Univ. of Sydney, 1891–1924 ; expedition to Ellice Is., 1897 ; Shackleton Antarctic exped., 1907–09, reaching South Magnetic Pole, 1909 ; wrote *The Geology of Australia*, 1932 ; geologist to British armies on Western Front, 1918–19.

DAVIS, William Morris (1850–1934), assistant in Central Observatory, Cordoba, Argentina, 1870 ; Harvard Univ. and U.S. Geological Survey, 1878 ; Professor of physical geography, Harvard, 1890 ; special professor of geology, *ibid.*, 1912, travelled extensively in America, Europe, Asia, South Africa, etc.

DE WINTON, Major-Gen. Sir Francis Walter (1835–1901), joined R.A., 1854 ; service in Crimea, North America, Near East, Africa ; secretary to Emir Pasha Relief Expedition, 1888 ; comptroller and treasurer to H.R.H. the Duke of York, 1892 *seqq.*; G.C.M.G., 1893.

DICKINSON, Basil Bentham (d. 1941), a Cambridge scholar in natural science, master at Sutton Valence school 1884–87, and afterwards at Rugby School, became hon. sec., Geographical Assoc. on its foundation, 1893, associated with Mackinder, Mill, Herbertson and others therein.

DICKSON, Henry Newton (1866–1922), worked under John Murray in Challenger office, under A. Buchan in Ben Nevis Observatory, and at Plymouth Marine Biological Station on sea temperature and salinity ; lecturer in physical geography, Oxford, 1899 ; professor of geography and director of dept. of commerce, University College, Reading, 1906–20 ; head of geographical section, Naval Intelligence Division, 1915–19 ; C.B.E., 1919 ; asst. editor, *Encyclopaedia Britannica* (12th ed.), 1920.

FITZROY, Robert (1805–65), F.R.S., vice-admiral in command of H.M.S. Beagle during Charles Darwin's voyage ; ethnologist ; head of Meteorological Dept., 1854.

FRESHFIELD, Douglas William (1845–1934), travel and mountaineering in Japan, Himalaya, Alps, Near East, Uganda, etc. ; president, Alpine Club 1893–95 ; Geographical Association 1897–1910 ; R.G.S., 1914–17.

GALTON, Sir Francis (1822–1911), F.R.S. ; Kt., 1909 ; gen. secretary, B.A., 1863–68 ; hon. sec., R.G.S., 1857–62 ; travelled in Sudan 1845–46, S.W. Africa, 1850, etc. ; studied and published works on meteorolgy, heredity and psychology, and founded science of eugenics.

GEDDES, Sir Patrick (1854–1932), studied zoology with T. H. Huxley and others ; senior demonstrator, University Coll., London, 1877–78 ; travelled in Mexico, 1879 ; formed sociological laboratory in Outlook Tower, Castle Hill, Edinburgh, 1892 ; prof. of botany, U.C., Dundee, 1889–1914 ; prof. of civics and sociology, Bombay Univ., 1920–23 ; founded student residence (the Scots College), Montpellier, 1924 ; writer on country and town planning, etc. ; Kt., 1932.

GEIKIE, Sir Archibald (1835–1924), F.R.S.; Kt., 1891 ; K.C.B., 1907 ; O.M., 1914 ; entered Geological Survey, 1855 ; director, Geol. Surv. of Scotland, 1867 ; Murchison prof. of geology and mineralogy, Edinburgh University, 1871–82 ; director, Geol. Survey, 1882–1901 ; president, Geol. Soc., 1891–92, 1906–08 ; British Association, 1892 ; Royal Soc., 1908–13.

GEIKIE, James (1839–1915), Geological Survey, 1861–82 ; followed his elder brother, Sir Archibald Geikie as Murchison prof. of geology and mineralogy, Edinburgh University ; worked on origins of surface features and glacial action.

GODWIN-AUSTEN, Lt.-Col. Henry Haversham (1834–1923), Trigonometrical Survey of India 1857–77 ; work on natural history and geology of India.

GOLDSMID, Sir Frederic John (1818–1908), Madras army; director-general, Indo-European telegraph, 1865–70 ; official journeys in Baluchistan, Afghanistan, Egypt and Congo ; orientalist ; K.C.S.I., 1871.

GRANT, James Augustus (1827–92), military service in India ; with Speke on exploration in east central Africa, 1861–63 ; C.B., 1866 ; Abyssinian expedition, 1868 ; Lt.-Col., C.S.I., 1868.

HERBERTSON, Andrew John (1865–1914), assistant to reader in geography (Mackinder), Oxford, 1899 ; reader, 1905 ; professor concurrently 1910 ; author of many textbooks, etc., and of *Atlas of Meteorology*, 1899 ; introduced scheme of major natural regions at an R.G.S. meeting, 1905.

HOGARTH, David George (1862–1927), archaeologist ; C.M.G., 1918 ; keeper of Ashmolean Museum, Oxford, 1909 ; Commander, R.N.V.R., 1915–19 ; director of Arab Bureau, Cairo, 1916 ; president, R.G.S., 1925–27.

HOLDICH, Col. Sir Thomas Hungerford (1843–1929), joined R.E., 1862 ; saw service on N.W. frontier of India and in Abyssinia ; Afghan Boundary Commission, 1884–86 ; Perso-Baluch boundary, 1896 ; Argentine-Chile boundary, 1902–03 ; president, R.G.S., 1916–18.

HOOKER, Sir Joseph Dalton (1817–1911), F.R.S.; K.C.S.I., 1877 ; G.C.S.I., 1897 ; assistant-director, Kew Gardens, 1855–65 ; director, 1865–85 ; president, B.A., 1868 ; president, Royal Society, 1873–78.

HUGHES, William (d. 1876), professor of geography at King's College, London, and afterwards at Queen's College ; numerous works on geography in various branches.

HUMBOLDT, Friedrich Heinrich Alexander, Baron von (1769–1859), b. Berlin ; travelled in Orinoco and Amazon basins, Andes, Mexico etc., 1799–1804 ; Siberia, 1829 ; naturalist, geographer and meteorologist ; author of *Kosmos*, 1845 *seqq.*

HUXLEY, Thomas Henry (1825–95), F.R.S.,

P.C., 1882 ; biologist ; Hunterian professor, Royal College of Surgeons, 1863–69 ; Fullerian professor, Royal Institution, 1863–67 ; rector, Aberdeen University, 1872–74 ; president, B.A., 1870 ; pres. R.S., 1883–85.

KANT, Immanuel (1724–1804), German philosopher, professor of logic and metaphysics in Königsberg, where he lived almost throughout his life, travelling very little, but wrote on cosmogony, astronomy, the moon, earthquakes, winds and the races of man ; his lectures on physical geography were published posthumously (1822).

KELTIE, Sir John Scott (1840–1927), editorial staff, Chambers, 1861 ; Macmillan, 1871–84 ; inspector of geographical education, R.G.S., 1884 ; librarian, 1885–92 ; secretary, 1892–1915 ; editor, *Statesman's Yearbook* from 1880 ; Kt., 1918.

KITCHENER, Horatio Herbert, 1st Earl (1914), of Khartoum and of Broome (1850–1916), entered R.E., 1871 ; Palestine survey, 1874–78 ; Cyprus survey, 1878–82 ; service in Sudan, Egypt, South Africa, etc. ; Field-Marshal, 1909 ; Secretary of State for War, 1914–16.

KROPOTKIN, Prince Peter Alexeivich (1842–1921), Russian geographer and revolutionary, carried out surveys in Manchuria and in 1873 published an important study of structural lines of Asia ; also investigated glacial deposits of Finland and Sweden. Embraced nihilism c. 1872, escaped from Russia, and lived subsequently in Switzerland, France and England.

LESSEPS, Ferdinand de (1805–94), French consular and diplomatic service, 1825–49, in Egypt and elsewhere ; conceived Suez Canal scheme, 1832 (concession, 1854 ; opened, 1869) ; designated head of Panama Canal scheme, 1879 ; scheme failed, 1888.

LIVINGSTONE, David (1813–73), missionary and explorer, in Bechuanaland, 1841 ; Kalahari desert and lake Ngami, 1849 ; Zambezi river, 1853 *seqq.* ; lake Nyasa, 1861 ; lake Tanganyika, 1867 *seqq.* ; joined by H. M. Stanley at Ujiji, 1871.

LYELL, Sir Charles (1797–1875), F.R.S. ; Kt., 1848 ; Bart., 1864 ; professor of geology, King's College, London, 1831–33 ; president, Geological Soc., 1849 ; president, B.A., 1864.

LYONS, Sir Henry George, (1864–1944), F.R.S. ; joined R.E., 1884 ; specialised in geology ; in Egypt 1890–1906, organised geological (1896) and cadastral (1898) surveys there ; univ. lecturer in geography, Glasgow, 1909 ; asst. director, Science Museum, 1911 ; director, 1920–33 ; president, Royal Meteorological Soc., 1915 ; temporary director, Meteorological Office, 1918.

MACKINDER, Rt. Hon. Sir Halford John (1861–1947), Oxford Univ. extension lecturer, 1885 *seq.* ; reader in geography, Oxford, 1887–1905 ; head of Oxford School of Geography, 1899 ; principal, Univ. Coll., Reading, 1892–1903 ; expedition to Mt. Kenya, 1899 ; director, London School of Economics, 1904–08 ; part-time reader in geography, Univ. of London, 1900–25 ; M.P., 1910–1922 ; British High Commissioner for South Russia, 1919–20 ; Kt., 1920 ; chairman, Imperial Shipping Ctee., 1920–45 ; P.C., 1926.

MARKHAM, Sir Clements Robert (1830–1916), F.R.S. ; K.C.B., 1896 ; Arctic expedition, 1850–1851 ; assistant secretary, India Office, 1867–77 ; pres., R.G.S., 1893–1905 ; pres., International Geographical Congress, 1894–99.

MILL, Hugh Robert (1863–1950), worked on *Challenger* Reports ; librarian, R.G.S., 1892–1900 ; director, British Rainfall Organisation, 1900–19 ; authority on Antarctic and adviser to explorers there.

MURCHISON, Sir Roderick Impey (1792–1871), F.R.S. ; Kt., 1846 ; K.C.B., 1863 ; Bart., 1866 ; served in army, 1807–14, and saw action in Peninsular war, etc. ; pres., Geological Soc., 1831 ; pres., R.G.S., 1843–4, 1851–2, 1856–9, 1862–71 ; pres., B.A., 1846 ; director-general, Geological Survey, 1855.

MURRAY, Sir John (1841–1914), K.C.B., F.R.S., voyage to Spitsbergen, 1868 ; appointed one of the naturalists for voyage of the *Challenger*, 1872 ; principal assistant and afterwards editor (1882–96) of *Reports* of the voyage ; explorations in Faeroe Channel and other Scottish waters, 1880–94 ; bathymetrical survey of fresh-water lochs of Scotland, 1897–1910 ; K.C.B., 1898.

NEWBIGIN, Dr. Marion (1869–1934), extramural lecturer in zoology, Women's Medical School, Edinburgh ; lecturer on plant and animal geography, Bedford College, London ; editor, Scottish Geog. Mag., 1902–34 ; travelled especially in Mediterranean lands.

OMMANEY, Adm. Sir Erasmus, F.R.S. (1814–1904), entered R.N., 1826 ; admiral, 1877 ; Arctic expeditions, 1838, 1850 ; discovered Franklin expedition's first winter quarters ; active service in Russian war, 1854–55 ; Kt., 1887 ; K.C.B., 1902.

PALGRAVE, William Gifford (1826–88), entered army and served in India, then joined church of Rome, ordained priest, Jesuit missionary in India, Syria and Arabia ; published *Narrative of a Year's Journey through Central and Eastern Arabia*, 1865 ; left priesthood and carried out government missions abroad ; resident minister in Uruguay, 1884.

PENCK, Albrecht (1858–1945), employed in geological survey of Saxony, 1877 ; academic positions in Munich, Vienna and Berlin ; travelled extensively : his life and works are fully reviewed in *Mitt. der geographischen Gesellschaft*, Wien, vol. 89.

PESCHEL, Oscar (1826–75), professor of geography Univ. of Leipzig ; a founder of physical geography as now understood ; his major works were published in 1870 and 1879.

PETERMANN, August Heinrich (1822–78), German cartographer, assisted Keith Johnston in Edinburgh with English edition of Berghaus' Physical Atlas ; director of Perthes' geographical institute, Gotha, 1854 ; editor, *Petermanns Mitteilungen* ; appointed physical geographer-royal by Queen Victoria.

RAVENSTEIN, Ernst Georg (1836–1913), born Frankfurt-am-Main ; migrated to London, 1852 ; topographical dept., War Office, 1855–72 ; compiler of maps of central and eastern Africa, etc. ; works on history of cartography.

REEVES, Edward Ayearst (1862–1945), joined staff of R.G.S., 1878 ; instructor in surveying and map curator there, 1900–33.

RITTER, Karl (1779–1859), published first edition *Die Erkunde in Verhältnis zur Natur und zur Geschichte des Menschen*, 1817–18 ; professor extraordinarius of history, Berlin and lecturer at military college, 1820–59 ; second ed. of *Erdkunde*, expanded, begun 1822, left incomplete.

ROXBY, Percy Maude (1880–1947), after study under Herbertson at Oxford, began teaching of

geography at Liverpool University, 1904, later becoming professor ; visited and studied China, 1912 and subsequently.

SCHLAGINTWEIT. Three of five brothers, German travellers, viz. Hermann (1826–82), Adolf (1829–1857), and Robert (1833–85), became well known to British geographers for scientific studies in the Alps *c.* 1850, and more especially for exploration in Deccan and Himalaya, notably in Karakoram and Kuenlun areas.

SCHOMBURGK, Sir Robert Hermann (1804–65), of German birth, explored British Guiana at instance of R.G.S., 1835 ; surveyed there and determined Schomburgk line, boundary with Venezuela, 1841 ; British consul, S. Domingo, 1848 ; Bangkok, 1857.

SCORESBY, Rev. William (1789–1857), F.R.S., followed his father in Arctic whale fishery ; studied arctic meteorology and natural history, also terrestrial magnetism ; charted 400 miles of E. coast of Greenland, 1822 ; took holy orders, 1825 ; magnetic research, Australia, 1856.

SCOTT, Capt. Robert Falcon (1888–1912), commanded National Antarctic Expedition, 1900–1904, and British Antarctic Expedition, 1910 ; met death during return march from South Pole.

SEWARD, Sir Albert Charles, F.R.S. (1863–1941), palaeobotanist ; university lecturer in botany, Cambridge, 1890 ; professor, do., 1906–36 ; master of Downing College, 1915–36 ; vice-chancellor, Cambridge U., 1924–25 ; president, B.A., 1939.

SHACKLETON, Major Sir Ernest Henry (1874–1922), National Antarctic Expedition, 1901 ; commanded British Antarctic Expedition, 1907 ; further expeditions 1914, 1921.

SPEKE, John Hanning (1827–64), explorer with Burton in Somaliland, 1854 ; great lakes of Central Africa, 1856 *seqq.* ; with Grant in east central Africa, 1861–63.

STANLEY, Sir Henry Morton (1840–1904), born in Wales as John Rowlands, was adopted by an American merchant whose name and nationality he took, 1859 ; fought in American Civil War, 1861 ; entered journalism and acted as correspondent in Asia Minor and elsewhere ; sent by *New York Herald* to meet Livingstone in Africa, 1871 ; subsequent extensive African travels especially in Congo area ; naturalized British subject, 1892 ; M.P., 1895–1900 ; G.C.B., 1899.

STRACHEY, Sir Richard (1817–1908), F.R.S., saw active service early in life in India, and from 1858 to 1889 held various high administrative posts there, concerned with irrigation, forestry, meteorology, and especially finance ; researches in physical geography and botany of Himalaya ; G.C.S.I., 1897.

STURT, Charles (1795–1869), entered Army, discoveries in central Australia on three expeditions between 1828 and 1845 ; surveyor-general in South Australia, 1833 ; later chief secretary till 1856 ; designated K.C.M.G., but died before investiture.

WALLACE, Alfred Russel (1823–1913), O.M., F.R.S., visited Amazon with Bates, 1848–52 ; Malay Archipelago, 1854–62 ; researches in zoology, botany etc., also on origin of species contemporaneously and on parallel lines with Darwin ; social science, &c.

WARREN, Sir Charles (1840–1927), F.R.S., entered R.E., 1857 ; special service in Palestine under Pal. Expl. Fund, 1867 ; active service in Kaffir War, 1877–78 ; search for Edward Henry Palmer's expedition from Suez, 1884 ; administrator, Bechuanaland, 1884 ; G.C.M.G., 1884 ; chief commissioner, metropolitan police, 1886 ; commander, Singapore, 1889–94 ; South African War, 1899–1900 ; archaeologist, etc.

WHYMPER, Edward (1840–1911), artist and mountaineer, made many first ascents in the Alps, 1860–65, notably that of the Matterhorn, 1865 ; expeditions to Greenland, 1867 and 1872 ; equatorial Andes, 1880 ; Canadian Rockies, 1901–05.

THE NATURE OF PROFIT

Address by

PROF. R. G. HAWTREY, C.B.

PRESIDENT OF SECTION F

IN the practical affairs of trade, industry and finance no concept is more fundamental or more familiar than profit. Yet to the questions what profit is, and by what causes it is shaped and determined, economic science has not as yet supplied answers which command general agreement.

If we go back to Adam Smith, we find in the *Wealth of Nations*, the germs of some of the ideas which have fructified since. He regarded profit primarily as the remuneration of capital. 'The value which the workmen add to the materials resolves itself into two parts, of which the one pays their wages, the other the profits of their employer upon the whole stock of materials and wages which he advanced.'

'The profits of stock,' he proceeded, 'it may perhaps be thought, are only a different name for the wages of a particular sort of labour, the labour of inspection and direction. They are, however, altogether different, are regulated by quite different principles, and bear no proportion to the quantity, the hardship or the ingenuity of this supposed labour of inspection and direction. They are regulated altogether by the value of the stock employed, and are greater or smaller in proportion to this stock.'

He supposes two manufacturers employing the same number of work people, but one using £1,000 of capital and the other £7,300. If 'the common annual profits of manufacturing stock are ten per cent.' one will have a yearly profit of only £100, the other £730. Yet 'their labour of inspection and direction may be altogether or very nearly the same.'

Adam Smith distinguished profit from interest. While 'the revenue derived from stock by the person who manages or employs it is called profit; that derived from it by the person who does not employ it himself, but lends it to another, is called the interest or the use of money.' Part of the profit made by the use of the money 'naturally belongs to the borrower, who runs the risk and takes the trouble of employing it, and part to the lender who affords him the opportunity of making this profit.

Adam Smith never ventured upon an explanation of how the rate of profit is determined, but he did arrive at a relation between profits and wages. The need for a minimum of subsistence put a lower limit to wages. But, if capital increased faster than population, there resulted an intensified demand for labour, and wages rose. If capital failed to keep pace with population, wages would fall below the subsistence level, and unemployment and starvation would result.

Ricardo substantially accepted Adam Smith's account. But he laid more stress on the subsistence standard of wages, especially on the tendency for a rise of wages above the subsistence level to be counteracted by the increase in population which it would evoke. And he enunciated the principle that there can be no rise in the value of labour without a fall of profits.

It fell to young John Stuart Mill in 1830 to take the prevailing doctrines a step further towards their logical conclusion. His *Essays on Some Unsettled Questions of Political Economy* contained one on Profits and Interest.

He pointed out that 'Mr. Ricardo's principle that profits depend upon wages, rising as wages fall, and falling as wages rise,' required careful interpretation. 'A rise of wages, with Mr. Ricardo, meant an increase in the cost of production of wages; an increase in the number of hours' labour which goes to produce the wages of a day's

labour ; an increase in the *proportion* of the fruits of labour which the labourer receives for his own share.'

Mill restated Adam Smith's principle that ' the difference between the profit which can be made by the use of capital and the interest which will be paid for it is rightly characterised as wages of superintendence,' but is not ' regulated by the same principles as other wages.' Of two traders, making a profit of 5 per cent. in excess of interest, one on a capital of £100 the other on £100,000, one would be remunerated by £5 and the other by £5,000. ' Yet it cannot be pretended that the labour of the two borrowers differs in this proportion.'

Here Karl Marx took up the story. And it is interesting to see how little of substance he added. He accepted the subsistence theory of wages from Ricardo, and drew the inexorable conclusion that the entire surplus of production over the necessities of subsistence must accrue to people other than wage-earners. The theories of rent and interest only showed how the surplus was to be divided among them. The land-owners were in a position to exact one part, the owners of capital resources another. The residue constituted the pure profits of exploitation received by those who made active use of the capital resources.

For the purposes of Marx's onslaught upon capitalism, it was not necessary to work out the principles underlying the division of the surplus. The existence of the surplus itself provided a sufficient text for his diatribes. But without theories of interest and rent, the surplus failed to provide a theory of profit. Before his death Marx had turned his attention to this problem, and it was dealt with in the posthumous volumes of *Das Kapital*. He had assimilated the Ricardian theory of rent, but the doctrine of marginal yield, as applied to interest and wages, had hardly gained acceptance in his lifetime, and he knew nothing of it. As to interest, he got no further than to say that it is a part of the surplus, which the user of capital gives up to the owner.

The case against exploitation depended on the subsistence theory of wages. According to Ricardo the growth of capital would mean an increasing wage fund and a tendency for wages to rise, but he insisted that the growth of population would in general overtake the growth of capital and bring back wages to the subsistence level. He did, however, temper this pessimism by allowing some elasticity to the standard of subsistence, and Marx likewise recognised that a subsistence wage must be understood to be a wage providing a customary standard. This admission was a serious threat to his position. For he regarded an excessive tendency to accumulation as the essential contradiction in capitalism, and, if accumulation raised wages, the way seemed open to an indefinite improvement of the customary standard. Nor would he admit that the working class were responsible for depressing their standard by multiplying their progeny. The guilt of exploitation must be fastened on the capitalist, and must not be shared with anyone else.

Marx found the solution in the application of a part of the capitalist's accumulation to labour-saving plant. The substitution of machinery for men would prevent a shortage of labour from bringing the use of the growing capital resources in exploitation to a dead stop, but it would not necessarily of itself prevent a rise of wages, perhaps far above susbistence level. Marx never really grappled with the question.

But he did take over from the classical economists two principles which contributed to a theory of profit. First he regarded the surplus (except rent) as divided among capitalists through competition in proportion to the capital employed by each, that is to say, he regarded profit as divided in the same proportion as interest.

Secondly, the active capitalist, as differentiated from the mere owner of capital who receives interest, is, as Marx put it, ' a simple agent of the labour process, a labourer and specifically a wage labourer.' ' This portion of surplus value is thus no longer surplus value, but its opposite, an equivalent for labour performed.' The industrial capitalist in fact receives ' the wages of superintendence.' But he is none the less ' an exploiter of the labour of others,' because the wages which he claims ' do not depend upon the degree of his exertions in carrying on this exploitation.'

Here Marx, like Mill, is echoing Adam Smith's example of the two manufacturers who receive 10 per cent. on their respective capitals, and, whereas one gets £100 a year and the other £730, yet 'their labour of inspection and direction may be altogether or very nearly the same.'

The industrial capitalist, says Marx, 'can easily shift this burden to the shoulders of a superintendent for moderate pay.' Marx after all had not departed from the position of Adam Smith, Ricardo and Mill.

But progress was being made in economic analysis. Marshall's *Principles*, appeared seven years after the death of Marx. The principle of marginal yield, evolved out of the Ricardian theory of the rent of land, had been extended by Jevons and others to wages and interest. If the shares of all the factors of production, land, labour and capital, had been fully explained, was not profit accounted for as the residue?

Not quite. According to the principle of marginal yield, industry would engage labour up to the limit at which the additional cost would no longer be more than compensated by the proceeds of sale of the additional output. In this calculation capital could be reckoned in as a substitute for labour, the payment of interest being such as to equalise the cost-saving capacity of the least remunerative plant employed with its cost. Labour and capital could thus be lumped together as costs.

In any such calculation a distinction has to be made between prime costs and overhead costs. A productive enterprise is planned and equipped for a certain output; its capital charges and the cost of management and administration become commitments which cannot be appreciably reduced when output falls below capacity. These are the overhead costs, while such expenses as those of materials and wages, which are substantially proportional to output, are the prime costs.

If the enterprise is to pay its way, it must receive a price for its product sufficient to cover prime costs and to contribute a margin towards overhead costs. When it is employed up to capacity, the margin must be sufficient to meet the whole of the overhead costs. It is this margin which raises the problem of profit. For the overhead expenses must include appropriate remuneration of the services of the entrepreneur himself. What he gets is the residue remaining after all the disbursements on overhead costs have been deducted, and it is not easy to establish any intelligible relation between the residue and the services which it is deemed to remunerate.

Marshall's treatment of profit had an apologetic flavour; he described profits as a share of the normal expenses of production, which could not long diverge from the 'normal supply price' of its components: of the capital needed; of the ability and energy required for managing the business; and of the organisation by which the business ability and the requisite capital are brought together.

Marshall recognised the risk of loss as an element to be allowed for in the calculation of profit, not only that losses must be set against gains in reckoning the average rate of profit received or anticipated, but that to a prudent capitalist uncertainty itself is a disadvantage for which he wants to be compensated. But he rejected the view that the whole of profit, after deducting a fair equivalent for earnings of management could be explained away as compensation for risk.

Compensation for risk is a cost. Along with interest and depreciation, it goes to make up the cost of using capital, and forms part of the total cost to be deducted from the proceeds of sale to arrive at the margin of profit.

The reckoning of compensation for risk did however reduce the extent of the residue to be explained, and entered prominently into Marshall's apologetics. It was probable, he contended, that the true excess of profits over interest was not on the average more than half of what it appeared to be to those 'who form their estimate of the profitableness of a trade by observing only those who have secured its prizes.'

Uncertainty in itself means inequality, and Marshall disentangled his treatment of normal supply price and normal profit from it by supposing a representative firm, one not exceptional either in efficiency or

in size or in luck. The true upshot of his apology was, I think, that the profits of the representative firm were not more than fair remuneration for the services rendered.

The anomaly which the classical economists found in profit was that the profit-maker's income, being proportional to the scale of his business, cannot correspond to the value of the work he does in the same way as a salary. When Marx said that the 'wages of superintendence' which the capitalist receives 'do not depend upon the degree of his exertions,' he was echoing Mill and Adam Smith.

If the productive concerns composing an industry tended to be all of a certain most economical size, so that any which are either greater or smaller were less profitable, we might suppose that competition would assure a reasonable income to those carrying on the concerns of the economical size, and that those too small would drop out or be merged, and those too big would either fail or be divided. There are some industries in which very big units are unsuitable. And in all enlargement beyond a certain limit begins to cause difficulties of administration which may outweigh the advantages of enlargement. Nevertheless the range of size is so wide that nowhere does a representative firm supply a standard size and a standard profit income. Marshall expressly excluded from his conception of the representative firm one 'which by exceptionally long-sustained ability and good fortune has got together a vast business and huge well-ordered workshops that give it a superiority over almost all its rivals.'

It was the concentration of industry in big units which Marx regarded as the essential characteristic of capitalistic exploitation. The small profit-makers, whom he classed as petty bourgeois, were destined to disappear under stress of competition, and become merged in the proletariat. Even the peasants he included by a distortion of language under the description petty bourgeois.

The concentration of industry in big units on the scale that mechanisation made possible was a recent development in Marx's day. But there had been from time immemorial a concentration which favoured no less the evolution of big fortunes and plutocratic power. The business of the merchant made its appearance in the remote past as soon as the division of labour called for the means of exchanging products.

The purpose of production is *to meet needs*; activities which do not meet any need cannot properly be called production at all. When a man produces not for his own needs but for those of other people, he must be able to make contact with those whose needs he is meeting. A market is an organisation through which he can do so. A market in a product brings together supplies from all sources in order to satisfy all sources of demand.

Marketing therefore is an essential part of the process of production. And it is a mistake to think of a market as automatically created by the mere volition of the buyers and sellers. A market is a substantial enterprise, carried on through the skilled services of those who compose it.

They are the intermediaries, the merchants and dealers, wholesale and retail, who buy in order to sell again. They are remunerated by the difference between buying price and selling price—in fact by profit. Their remuneration has the special characteristic of profit, that it can be multiplied in proportion to the magnitude of the recipient's transactions. That is why, in the ages before there was large-scale industry, the successful merchant was the typical capitalist.

I have said that the merchants and wholesale and retail dealers in goods render skilled services. What is it that they do? Whatever is produced comes forward with a description or specification indicating what need it is to meet and how. It is only within a limited field that the consumer himself takes the initiative and orders a producer to supply something according to his own specification. In general the producer has to anticipate the consumer's need. The consumer, when he buys, selects from a stock of things already produced, which the retailer has at hand.

If the retailer is to give his customers an adequate choice, he must get supplies from many sources. At the same time a producer will want to supply many retailers. The merchant intervenes to

buy goods from the producers and distribute them among the retailers. Likewise the producer, who wants materials and intermediate products to be used in a further stage of production, procures them through a merchant who has access to many sources of supply.

If the specifications of products were always perfect and adequate so that the consumer or user could always get exactly what he wanted by giving the trade description, the process of suiting products to needs would be one of routine, a mere clearing of demand against supply. Demand could always be unmistakably formulated, and producers would know how to furnish the supply.

But even so the business of marketing would not be merely mechanical. For the demand for any product would fluctuate, and the traders who supplied it would have to hold a stock. The merchant's stock of goods destined for sale is the major part of the capital of his business. The holding of it is a part of the service he renders to both producers and consumers. For the consumers the existence of a stock of goods ready for use ensures the immediate satisfaction of demand. For the producers the stock is a cushion absorbing fluctuations in demand. The merchants have the responsibility for foreseeing demand and adjusting the orders they give to the producers accordingly. Thereby they make possible a continuity of production modified only by adjustment to considered forecasts of demand.

But the supposition that specifications and descriptions are so perfect that products fall into rigidly defined categories is far from the facts. The name of an article is a first approximation. The purchaser, whether a consumer who wants a finished product, or a producer who wants materials or equipment, can supplement the name with his own more specific description. But the best description he can give will leave room for choice, and it is for the seller to offer a suitable selection for the buyer to choose from.

It is in procuring this selection of goods to suit the needs of the consumers and users that the merchant's skill chiefly lies. He must appreciate the refinements of their needs, and must be able to distinguish the qualities that will suit them among all the variety of goods which the producers can supply. He can to some extent influence what is produced, for when he orders goods from a producer he furnishes a specification. But the specification has to be limited to the kinds of things the producer is equipped to produce.

Thus a part of the merchant's responsibility for suiting needs is passed on to the producers. The orders the producers receive from the merchants reflect the merchants' own sales, and ultimately the demand from the consumers, and the producers are thus led to adjust their output of any product to the demand for it. A producer, being concerned with selling his own product, can facilitate the purchasers' choice by ensuring uniformity in the product and attaching a trade name or brand to it. The purchaser, by quoting the trade name, will circumvent the difficulties of specification and will know just what he is getting.

The greater part of production has to be planned far ahead. When plant is installed for a productive process, the expectation is that the demand for the product at a remunerative price will last as long as the plant. The governing consideration is the prospect of demand.

The industrialist is no less a seller than the merchant, but what he sells is limited to his own product, and whereas the merchant can turn over his operations at short notice from one product to another, within the limits of those in which he has the necessary qualifications to trade, the industrialist commits himself to a long-range forecast.

The industrialist's operations are limited like the merchant's by his sales. But whereas the merchant's profit is the margin between buying price and selling price, the producer's profit is the margin between selling price and costs. He is faced with the twofold task of producing the product and selling it. Economists have tended to treat profit rather as the remuneration of efficient and economical production than as the remuneration of selling. And of course, if the producer can reduce his costs without deterioration of quality, he reaps his reward in a bigger margin between costs and selling price, that is to say, in increased profit. So much of his profit can be described as earnings of

management or of organisation. But this gain still has the characteristic of profit that it can be *multiplied* in proportion to the magnitude of the business, in other words, in proportion to sales.

The producer need not himself do the work of organisation and management. He may entrust the whole technical side of his concern to a salaried staff, and show his own competence in the judicious selection of them. The cost of the staff becomes part of his overhead expenses, and then the profit margin left to him can no longer be termed remuneration of management.

In fact for the producer no less than for the merchant, profit is the remuneration of *selling*. The responsibility for selling attaches inseparably to the *owner* of the product to be sold.

Usually the owner of the fixed capital of any concern is the owner of the product. But that is not necessarily so. A farmer may hold his land and all its fixtures on lease from a landowner, and may even be using hired plant, but, so long as he is the owner of the produce, he receives the profit. Or a retailer may be the tenant and not the owner of his shop.

For Marx the typical profit-maker was the owner of a large-scale producing concern, and he regarded the profit as the fruits of the exploitation of the labour employed. In reality it is not the employer as such who exploits labour, but the seller of the product. And the exploitation consists in the characteristic of the profit-maker's income, that it can be multiplied in proportion to the volume of his sales. Marx's case against the exploiter was ultimately that his remuneration ' does not depend upon the degree of his exertions.'

The multiplication of profits has that effect, because the margin of profit in any trade must be sufficient to secure a reasonable income to the small-scale trader, and may therefore provide a much more than reasonable income to the large-scale. Governments seeking to apply a system of controlled prices and profits have over and over again come up against this dilemma : either they must fix prices so low that the small trader cannot support himself on the profit margin, or they must fix them so high that the big trader makes a plutocratic income.

And this problem throws light on the manner in which profits are determined under competitive conditions. According to orthodox doctrine competition ought to prevent excessive incomes from occurring. If in any trade those already established are making excessive profits, the prospect of gain will attract new-comers, whose competition will reduce selling prices and profit margins. But the attraction to the newcomers will be limited by the amount of business they can expect to get. The limit will be set partly by the amount of capital they are in a position to bring, and partly by the business connection they can obtain, in other words their prospect of sales. Competition tends to set up a normal *rate* of profit, calculated to offer a reasonable income to the potential competitor.

Actual rates of profit vary widely, and may depart far from the normal. It is because the variations are so great, that profit is sometimes stated to be the compensation for risk. Whoever either buys or produces goods for sale runs a risk, in that he may fail to sell them at a remunerative price. He may not merely fail to obtain a margin towards his own remuneration, but may incur losses which diminish his capital. In an extreme case he may lose the whole of his capital.

But the occurrence of losses merely illustrates the general principle that profit is the remuneration of selling power. If the selling power fails, there is no profit, or profit becomes a negative quantity. It is sometimes argued that, when losses are set against gains, the average profit margin works out at no more than interest on capital, and that the excess of profit over interest in the successful business is no more than compensation for risk. More plausibly it is contended that the average return to the owner of capital must include not merely the actuarial equivalent of the prospect of loss, but some compensation for the uncertainty itself. To the individual the advantage of gain of a given amount is by no means the equivalent of the disadvantage of loss of the same amount.

No doubt the newcomer or potential competitor in any trade makes these calculations. But he must in effect compare his prospects in business with the

income he might make in a profession or in salaried employment, along with interest on his capital placed in a secure investment. The prospects will not attract him unless, making all due allowance for the risk of loss, the profit he hopes for will yield a better income for himself. He must expect something in excess of the compensation for risk, though of course it is the very nature of risk that he may be disappointed. Even if there were such general miscalculation among all those seeking profit that in the end their average margin above the allowance they reckoned on for risk turned out to be zero, it would still not be correct to identify their profit with compensation for risk.

But can the shareholder in a public company, who receives a better yield on his capital than the holder of a gilt-edged security, regard the difference as anything other than compensation for risk? He may take no part whatever in the affairs of the company. But, being part owner of the company, he is part owner of its products and has an owner's interest in their sale. What is the passive shareholder being remunerated for?

A business is probably started as a partnership or a private company, by a few people who between them contribute the skill, knowledge and experience and the initial capital resources required. The business will consist in producing or buying certain products for sale. The sales are hoped for from some *new* demand: either there is a growing demand in some existing trade, which the established suppliers are not adequately meeting, or the new business relies on some innovation —a new product or a new process. If the promoters' hopes are realised, the sales are effected at a remunerative price.

They assume that they can procure the goods (by production or purchase) at not less cost than their competitors, and if they fail in that respect, whether through incompetence or ill luck, their venture may come to grief. But the basis of the enterprise is the sales, and if it is successful it emerges with an established connexion with purchasers whose needs it is able to satisfy.

The multiplication of the gain from profit is subject to the twofold limit: selling power, and capital. When selling power expands, and capital becomes the operative limit, the time has come for an extension of capital. That is the stage at which transformation into a public company becomes appropriate. The promoters are then in the position of selling the prospect of profit to the new shareholders. They can, if they please, sell the whole business for cash, and use the proceeds in a new venture; or they can retain their own shares, and take an active part in the future of the concern.

A marketable share is an investment, competing in the market with other securities, and its price should be the capitalised equivalent of its expected yield, with due allowance for risk. Risk in this context is an ambiguous term. The intending purchaser of shares can reduce the risk by himself studying the prospects. If he is competent to do so, and receives good advice, his task may be reduced to a residue of pure chance, and he may make a selection of investments with a yield well above the average.

The excess yield is pure profit. It accrues to him as the remuneration for making a wise selection among profit-makers to whom to tie his fortune. It will be observed that he acquires a continuing return for a single decision. Its continuance depends indeed upon the activities of those who actually carry on the business. But it is characteristic of the selling power from which profits are derived that, once acquired, it is relatively easily maintained. That is why the goodwill of a business is a saleable property. It also explains the disparity between the effort and the reward, to which Adam Smith, Mill and Marx all called attention. A manufacturer who puts a standardised article on the market puts a brand upon it, and advertises it. Provided quality and price really suit purchasers, and the quality is maintained, the time comes when relatively little pushing is necessary in order to sell his output.

The investor who makes a wise choice of shares once and for all, and enjoys a continuing high yield in the future, may be compared to a landowner who makes a wise decision on the use to be made of his land, and enjoys the rents thereafter. The investor and the landowner alike derive their incomes from the economic activities

of other people. The capital of the one and the land of the other afford opportunities of profit-making to those who actually use them.

This aspect of the shareholder's decision is especially conspicuous at the initiation of an enterprise or at its first flotation as a public company, when the value of the shares has not yet been tested by dealings in the market. The success of the enterprise depends on the sale of its product. The first shareholders become committed to being the owners of the product, and thereby assume the responsibility for its disposal, but they delegate the work of selling to the active managers of the enterprise. It is in virtue of their judgment in making this delegation that they look for something more than bare interest on their capital and compensation for risk.

Profit then is the remuneration of selling power. Every market establishes a margin between cost and selling price. For a merchant or dealer cost means buying price; for a producer cost means cost of production. The margin has to be such that, after covering interest and risk in respect of the capital engaged, and overhead expenses, including remuneration for the services of the owner of the business, where he takes the place of salaried staff, there remains a residue. The residue is the remuneration of selling power, and accrues to the owner of the business in his capacity as owner of its product.

Here is the solution of the problem that puzzled Marx: how could he reconcile the tendency of accumulation to raise wages, and to reduce the gains of the capitalist indefinitely, with his doctrine of exploitation? For employers are not willing to go on expanding output and engaging labour up to the limit at which the proceeds of an additional unit of output no more than cover the additional unit of cost. Their demand for labour stops short at the point where the proceeds secure them a normal margin above costs.

The margin is the minimum that secures a reasonable income to the small-scale trader, and of course there are always numerous profit-makers whose incomes are not above the reasonable level, and are fairly comparable with incomes derived from salaries or professional fees. Not only are there trades and industries where small businesses predominate, but even of the profits of big business a considerable proportion goes to small or moderate shareholders.

Yet the profit-making system does remain an anomaly. The growth of giant businesses in which Marx foresaw the climax of capitalism, has progressed since his day. From the crowd of small and moderate shareholders there stand out leaders who not only receive disproportionate incomes, but exercise a disproportionate command of economic power through their active control of capital resources. Profits are the principal source of saving, and much of the saving takes the form of reinvested profits remaining in the hands of those who manage the business.

The concentration of industry in large units is attributable in part to the economies of production on a large scale, but only in part. In most industries there is a limit of size at which these economies begin to taper off; and even if they continue at all, they may be outweighed by the burden of a top-heavy administrative organisation. But when concentration of production has reached its limit, the concentration of selling power may go much further. A great combine may own many separate productive enterprises, perhaps making a wide variety of products, its unity residing in its selling. Or there may be many independent producers marketing their product through relatively few large wholesale dealers or merchants.

A big selling organisation acquires something approximating to a monopoly. Even when there are several in one trade, ostensibly in competition with one another, the competition is usually not very active. It is not to the interest of any one of them to start undercutting the others, with the certainty of provoking retaliation from them. They tend to settle down with a price policy which just assures them a normal profit, and is low enough not to be too attractive to would-be new competitors in the trade. In fact the growth of near monopolies is by no means so serious an abuse as it is commonly represented to be. The big concern can afford to be content with a narrower profit margin than the small, and, when monopolistic practices are attacked, the accusation is usually not that their prices

are extortionate but that their treatment of smaller competitors is harsh and oppressive.

Two views of profit were latent in the classical economics of Smith, Ricardo and Mill : on the one hand, the beneficent invisible hand which led the self-seeking profit-maker to meet his neighbours' needs ; on the other, the subject of contention between employers and wage-earners. The two views survive in the controversies of the present day. In disputes between capital and labour the crucial question is whether the employers can spare from their profits the cost of what their workpeople ask for. What they can spare is limited by the need to retain a sufficient margin for their own remuneration.

The defenders of profit-making rely on the need for an incentive as their principal plea. And the force of the attack is in the contention that the incentive is out of due proportion to the efforts to be rewarded. Even when profit margins are very narrow, multiplication may still give rise to excessive individual incomes. And as many successive sales intervene between the producer and the consumer, the total share of the final proceeds of sale accruing to profit even from apparently narrow margins may be considerable.

Profit-making has long been under attack. The attack gained force in face of the outburst of profiteering during and after the First World War, when the working of markets was distorted by monetary inflation. But it had started long before. Nor was it exclusively the work of Marx and the Socialists. Marshall's apologetic attitude was not, I think, directed mainly towards Socialist and Communist criticism of profits, but to more widespread misgivings.

In the present century the Marxist attack has developed, and has become one of the dominant issues in the world. But profit-making has also been assailed from within the capitalist system. The advance of democracy has given an equalitarian bias to taxation. The successful profit-maker is allowed to amass his big income, but has to yield up the greater part of it to the tax-gatherer. It is assumed that what is left to him will be a sufficient incentive for him to devote his skill and his zeal to his business. Whether that assumption is justified is a question which I need not pursue.

In conclusion I would point out that proposals for ridding society of the abuses of profit-making should make some alternative provision for the services which the profit-makers render. Those who favour nationalisation, whether of all industry, or of certain selected industries, are rather apt to assume that the State in acquiring an industry takes responsibility only for the technical business of production. That is too limited a view. The producer takes the responsibility for understanding and suiting the needs to be met by his product. For private enterprise the test of success in doing so is the sale of the product. Enterprise really means enterprise in meeting a demand. There is a danger that nationalised enterprise will take the easy road of imposing standardised products on the consumer, and will blindly obliterate refinements by which material welfare is raised above an elementary minimum. When public policy intervenes in industry, whether by nationalisation or by planning, it is inclined to favour standardisation and mass production, because these methods of production contribute to a war potential.

When the big incomes of profit-making are attacked not by nationalisation but by direct taxation, the danger is not so much in the impairment of incentive as in the adverse effect upon saving. Profits are the principal source of saving ; not only the reinvested profits of businesses, but the profits and dividends distributed to individuals. For it is the recipient of a relatively large yet fluctuating and possibly precarious income who has the strongest motive to save.

Since the end of the war Chancellors of the Exchequer have recognised that a big budget surplus as a form of compulsory saving is the corollary of equalitarian taxation, though at the present time the burden of rearmament has come into conflict with this policy.

TWO THOUSAND YEARS OF ENGINEERING

Address by

SIR CLAUDE GIBB, C.B.E., F.R.S.

PRESIDENT OF SECTION G

ENGINEERING is much older than any history that has come down to us, for it stretches back to the beginning of civilisation; when man, during his gradual development from a barbaric savage, fashioned tools and weapons to obtain food, and to protect himself from the beasts of the forests.

In speaking of these prehistoric times, it has been customary to refer to the Stone Age, the Bronze Age, and the Iron Age, each being based on the material which was most prominently used for tools and weapons.

With the growth of the first civilised countries, problems arose in the cultivation of the land, and in transport. Engineers were called upon to provide irrigation schemes, reservoirs, canals, docks and harbours. These engineers relied chiefly on the force of human muscles for their power, and commanded many thousands of persons to do their will. The rulers of the day required the engineers to build wonderful palaces, and, after their death, great monuments, examples of which are the Pyramids. In addition, temples had to be erected for the worship of the gods, and these in their turn, presented great problems for the engineer. To shape the large blocks of stone used for the construction of these buildings, tools had to be designed and built, and here again, the engineers displayed much skill and ingenuity.

During the third and second centuries B.C., ancient Greece furnished us with a number of eminent engineers, who carried out many engineering feats, the written records of which have been passed down to us. Ctesibius Alexandrinus contributed to our knowledge of the properties of the wind and of pneumatic power. He invented a two-cylinder force pump, the remains of one being in the British Museum. Hero of Alexandria was possibly a follower of Ctesibius, and his researches brought him into the fields of mechanics, catoptrics, pneumatics, surveying, geometry and stereometry. To the engineer he is remembered for his original idea of a reaction turbine, and also for his hydraulics. The modern surveyor owes his theodolite to Hero, who called his own instrument the dioptra. Archimedes was another Greek genius who laid the foundation for our knowledge relating to specific gravity, and to the principles of leverage and the water screw. The mathematical problems which he solved are of vital importance to the engineer to-day, and he has, in fact, been called the great ancestor of modern engineering.

The development of the Roman Empire set the engineers great tasks. We still admire to-day those ancient buildings which have been preserved. But the greatest achievements of Roman technical work was the making of roads. Rome's military leaders covered the empire with a network of highways, examples of which are to be found in England to-day.

With the decline of the Roman Empire very little progress in engineering was made until the year 1629, when Giovanni Branca of Rome, published information on a new steam engine. This engine was, in fact, a steam turbine which operated on the impulse principle.

About this time coal mines in Great Britain were becoming deeper, and difficulties were being experienced in the pumping out of water, and the raising of the coal. Engineers turned their attention to the design of an engine for this purpose, and in 1663, the Marquis of Worcester, described a method of raising water by the use of steam. During the

next century the reciprocating piston engine made its appearance, and the names of Papin, Savery, Newcomen, Potter, Beighton, Smeaton and Watt, will always be associated with its development.

Progress in all branches of engineering from now on developed rapidly. Steam engines were fitted into river boats in this country, in America and in France. In 1819, the *Savannah*, a vessel of 300 tons was fitted with a single cylinder steam engine driving paddle wheels, and it crossed the Atlantic from America to Europe. This little vessel demonstrated the possibility of crossing the Atlantic by using steam as the main motive power, but some years had to pass before vessels and machinery of adequate strength and power were available to inaugurate the trans-atlantic service.

Henry Cort had discovered in 1783 a process for producing wrought iron by puddling, and of rolling it into plates and bars. This invention provided a new material for engineers, who quickly adopted it for the construction of ships, boilers, steam engines and bridges. The history of iron ships was commenced in 1787, when an iron canal barge was launched at Willey. The first iron steam vessel was the *Aaron Manby* which David Napier navigated from the Thames to the Seine in 1822. Screw propulsion was also introduced about this time and presented many difficulties. The pioneers had to solve problems relating to the best form of screw, the means of driving it fast enough, and how to take the thrust.

During the time these developments in marine propulsion were proceeding, engineers were also concentrating on improved land transportation. Some cheap and quick method of transporting coal from inland collieries to the river shipping points was required, and colliery engineers were tackling this problem. Trevithick, Blenkinsop, Hedley and George Stephenson will be remembered for their work in constructing steam locomotives for coal haulage. Stephenson also constructed the locomotive which drew the first public passenger train in the world, when it went into service on the Stockton and Darlington Railway in 1825. It was from this beginning that the modern railways in this country and throughout the world owe their heritage.

These developments in the steam engine had been made possible by the work of the engineers and physicists who did preliminary work on determining the laws of steam. Watt discovered that the latent heat of steam is nearly constant for any pressure within the range of steam engine practice, and that, consequently, the greater the steam pressure, and the greater the range of expansion, the greater will be the work obtained from a given weight of steam. He also invented the steam engine indicator. Count Rumford carried out experiments on frictional heat, and in 1824, Sadi Carnot published his book which gave us the pressure temperature cycle. Carnot's ideal diagrams with isothermal and adiabatic lines represented very closely the action of the steam in engines of that period, but he failed to recognise that heat disappeared in the process of producing power in an amount equivalent to the external work done.

Joule in 1849, established the indestructibility of energy, the mechanical equivalent of heat and the existence of the absolute zero of temperature. In the following year, Sir William Thomson (afterwards Lord Kelvin), deduced the absolute zero of temperature from Carnot's cycle and reached the same conclusion as Joule. Sir William Thomson, Rankine and Clausius continued the work by giving the subject mathematical treatment, and it is to these physicists that we are indebted for the discovery of the laws of thermodynamics, and to Clausius for the introduction of the term ' entropy.'

The beginning of the nineteenth century was also an important period in the history of electricity. Although magnetism had been known since ancient times by the Chinese and the Greeks, little use was made of it, except in connection with witchcraft, quackery and superstition. One of the first references to its use for practical purposes occurs in the description of Columbus's first voyage to America in 1492 when he used a compass.

Little was achieved during the next 300 years until Volta discovered in 1799, that electricity could be obtained by chemical action, and produced the voltaic cell giving a steady low-tension current.

Rapid progress in electrical research was thus made possible, resulting in discoveries by Oersted, Ampère, Faraday, Davy, Sturgeon, Ohm, Henry and finally Faraday again in 1831, when he made the greatest contribution to the development of electrical science by constructing the first magneto-electric machine. Further developments were made by other inventors, and by 1844, one of the first effective industrial electric generators was constructed for an electro-plating works at Birmingham.

Another field of electrical development was that of the electric telegraph. Attempts had been made in the eighteenth century to introduce the electric telegraph, but it was not until 1837 that success was achieved independently in the U.S.A. by Samuel Morse, and in England by Wheatstone and Cooke, who installed in 1843 the first public electric telegraph line. This was between Paddington Station and Slough.

By the year 1850, reciprocating steam engines were in general use both on land and sea ; the preliminary laws of thermodynamics had been established ; the steam locomotive was in use and passenger train services were being inaugurated in different parts of the country ; important developments had been made in electricity ; electric generators had been constructed, and the electric telegraph services were in use ; wrought iron had been discovered and was being used for the construction of boilers, steam engines, ships and bridges.

This then was the position a century ago when the stage was set for rapid developments in all branches of engineering.

1851–1951

With the success of the electric telegraph on land the first submarine cable was laid in 1850 between Dover and Calais, and after many attempts a trans-atlantic telegraph service was established in 1866 between Ireland and Newfoundland. The latter was operated by the Atlantic Telegraph Company. In the early days the transmitting speed was 15 letters per minute, and this has been increased to over 2,500 letters per minute.

The transmission of actual speech was next attempted, and although efforts had been made previously it was not till 1876 that a practical telephone was invented by Alex Graham Bell of the U.S.A. It was at the British Association Meeting held at Glasgow, in 1876, that Graham Bell in conjunction with Lord Kelvin brought to the Association's notice the telephone. Within a few years telephones were in use in all the advanced countries of the world.

If the telegraph and telephone changed the world by making possible instantaneous communication over long distances, even more revolutionary was the transmission of power by electricity. The first proposals for electrical transmission concerned electric lighting, and it was this development that led the way to the transmission of power.

The electric arc-light first demonstrated by Humphry Davy in 1809 had been turned into a practical commercial possibility by Foucault in 1844, but, while it was used for lighting railway stations, docks and for other outdoor purposes, it was not ideal for normal indoor lighting.

Many inventors attacked the problem of making an incandescent electric lamp but without success, and it was not till 1878 that Sir Joseph Wilson Swan made such a lamp and exhibited it at Newcastle. T. A. Edison was also working on the problem and he patented his process in the U.S.A. and in Great Britain in 1879. Companies formed by the two inventors were amalgamated in 1885 to form the Edison and Swan United Electric Light Company. The first opportunity given to the public of seeing electric incandescent lighting on a large scale was at the Crystal Palace Electrical Exhibition, which was opened in 1885.

Edison did much more than produce the incandescent electric light. He saw that the possibilities of such lighting were dependent upon the efficient supply of electric current from a generating station, and set himself the task of designing a reliable dynamo. In 1879, he produced a dynamo to give electric current at a constant pressure of 110 volts. These dynamos were designed to be driven either by a pulley and belt, by a steam engine, or else by coupling the armature shaft directly to the crank shaft of a steam engine. Edison also worked out the, by

no means obvious, system of distribution by cable and wiring circuits.

The first public lighting supply was opened at Appleton, Wisconsin in 1881, and in the same year the first station in England was opened at Godalming in Surrey. Then in the following year the much larger Pearl Street Station, New York, was opened with six generators with an eventual lamp connection of 6,000 to 7,000 lamps. By the end of the year nearly eighty electricity companies were founded in England, though few had actually gone into operation. In 1883 the first public electric railway was opened between Portrush and Giant's Causeway in Ireland.

When central station electric lighting by direct current was first established it was found that there were many towns and places where the demand was so scattered that it did not pay to put down the heavy feeders and distribution mains required on the D.C. system. Attention, therefore, became directed to the possibilities of alternating current supply. Many different types of alternators were designed by Gramme, Westinghouse, Ferranti, Kapp, Parker, Mordey, and Messrs. Ganz of Budapest.

Ferranti especially devoted his attention to the design and construction of large alternators for public electric supply, and installed a number of them in power stations in London. The Deptford Station will always be associated with the name of Ferranti, for it was there that he gave the lead to the world by installing single units of 10,000 H.P. generating at the unheard of pressure of 10,000 volts.

In the early days of electric lighting it was usual for the generating unit to be driven by a belt drive from some kind of steam engine almost invariably combined with its own boiler. Semi-portable engines of the agricultural type were generally employed and there was a serious need for some form of high speed engine for direct drive. One of the earliest high speed direct coupled engines was made by Peter Brotherhood who arranged his three cylinder engine to drive a dynamo directly from either end of the crank shaft. This was soon superseded by the Parsons Epicycloidal Steam Engine of 1877 which in turn was outclassed by the Willans central valve engine. The latter was displaced by the Bellis double acting high speed compound engine. While this evolution of the high speed reciprocating steam engine was taking place, the final solution, namely the steam turbine, was being brought to practical success by Sir Charles Parsons. He patented his first turbo-dynamo in 1884, and it was this little machine of only 7½ kW. which led the way to the large central station turbo-alternators built to-day. Parsons developed the turbine chiefly in relation to electricity generation, and many small turbo-generators were built for supplying current for lighting purposes in ships, factories, hotels, and so on. After the incorporation of condensers in 1891, the turbine rivalled the reciprocating engine in efficiency and was rapidly adopted for central station work. Progress was made in size and efficiency, and by 1912 turbo-alternators for an output of 25,000 kW. had been constructed, while to-day single units of over 200,000 kW. are in operation.

The commanding position of the steam turbine is due mainly to its high thermo-dynamic efficiency ; in large sizes it gets more work out of coal than can be got by any other prime mover, due to its ability to take full advantage of the high pressure high temperature steam conditions now in use, and the adoption of regenerative feed heating in which steam is tapped from the turbine at successive stages of the expansion to heat the feed water on its way back to the boiler.

Parsons also attacked the problem of adapting the turbine to marine propulsion, and after many disappointments was equally successful. He constructed a small launch, the *Turbinia*, in 1894, which was fitted with a single turbine driving one propeller. On trials the speed attained did not give the expected result due to the phenomenon known as propeller cavitation. New turbines and new propellers were fitted which ultimately enabled a speed of 34 knots to be obtained. During the Naval Review at Spithead in 1897, the *Turbinia* was demonstrated before a vast fleet, representing not only the might of the British Navy but the sea-power of other leading nations as well. Her performance at the Review focussed the attention of the Admiralty on the

possibilities of turbine propulsion, and they entrusted Parsons with the construction of a 30 knot turbine destroyer H.M.S. *Viper*. In the following years the turbine was tried in various vessels of the Navy with such success, that in 1905 a Committee on Naval Design appointed by the Admiralty advised that in future turbine machinery should be used exclusively in all classes of warships.

Meanwhile the first turbine-driven passenger vessel the *King Edward* was built in 1901 for service on the river Clyde ; this was followed by cross-channel boats, and later by vessels for the Liverpool —Canada passenger service. In 1906 the *Lusitania* and *Mauretania* were each fitted with four turbines totalling 70,000 H.P. The success of the turbine at sea permitted vessels to be driven at speeds that had previously been impossible. To-day the turbine is the recognised prime mover for all the Navies of the world, as well as for all the fastest ocean liners. It made possible the construction of those floating marvels the *Queen Mary*, *Queen Elizabeth*, *King George V* and many others. The introduction by Parsons of gearing between the turbines and the propellors enabled the size of machinery to be reduced and increased its efficiency.

To use fuel directly in a cylinder instead of the more usual cycle of fire, boiler and cylinder attracted the attention of inventors from the earliest days, but the arrival of the practical steam engine put an end for a time to work in this direction. It was not till the nineteenth century when coal gas became available that inventors once again took up the subject. Coal gas provided a suitable and readily available fuel, and in 1823 Samuel Brown constructed the first commercial gas engines. Progress after this became rapid due to the attention of many more inventors till 1876 when Otto produced the famous ' Otto silent gas engine ' completing its cycle in four strokes. Two years later Dugald Clerk produced his two stroke engine which for large powers tended to be the more favoured engine. Although large units using blast-furnace gas were developed with efficiencies greater than steam in suitable circumstances, yet to-day it is regarded merely as a step in the evolution of the petrol and oil engines. Petroleum in large quantities had been discovered in 1858 in the U.S.A. and the increasingly wide distribution of petroleum oils, particularly the paraffin oils and lamp oils focussed attention on the possibility of using oils as fuel instead of gas. The earliest oil engine to achieve success was the Priestman oil engine introduced in 1885, followed the next year by Daimler's engine which used an oil so volatile that a carburettor would serve to charge the incoming air with combustible vapour and which ran at high speed enabling bulk and weight to be lessened and power increased.

The Daimler engine marks the beginning of the modern petrol engine, but a good number of years had to pass before it took the dominant position it holds to-day.

Many inventors contributed towards the development of both the oil and petrol engine, one of the most notable being Rudolph Diesel, who in 1895 produced an engine, while able to use crude oil, gave a remarkably low fuel consumption and high efficiency performance. The diesel engine was developed for use on heavy road transport vehicles and in the larger sizes as a rival to the steam turbine for marine propulsion where it found many advocates. Much attention has also been paid to its use on railway traction, and the number of diesel locomotives now finding employment on main line working is growing rapidly.

The effects of the petrol engine were most revolutionary in the field of transport, first on the roads and then in the air ; its lightness in relation to power made it pre-eminently suitable for this purpose. Daimler at first intended his engine to be used for stationary work and for the propulsion of boats, but in 1886 he fitted it to a bicycle. The results were so successful that three years later he built another engine for a road vehicle. This was the beginning of the great motor industry. The year 1896 saw the founding of the English Daimler Motor Company and in the same year the Ford Motor Exhibition was held in the Crystal Palace, London. These early cars were frequently unreliable, but the mechanical design was quickly improved as a result of practical experience on the road and cars became less troublesome. Other improvements

such as better systems of ignition and cylinder cooling, pneumatic tyres, and streamline body construction have done much to popularize the motor car. It has been recorded that by the year 1928 the output of high speed internal-combustion engines exceeded by more than ten times the total horse-power of all power stations, ships and railways. Again at the World Power Conference, held in Berlin in 1930 it was stated that the number of motor cars on the world's roads was some thirty millions, with an output of at least 600,000,000 horse-power. To-day when nearly all horse drawn vehicles are off our roads, and self propelled vehicles are being used for practically all forms of transport including tradesmen's motor delivery vans, fire engines, ambulances, farm tractors and the many thousands of vehicles used by the military authorities, who can say what the total horse-power is now.

One of the most notable applications of the petrol engine is its use in aviation. Mans efforts to fly dates back to the earliest days, but it was not till about the seventeenth century that flight was achieved by the use of hot air balloons. Thereafter considerable experimental work was undertaken with power-driven model planes, the first model to rise under its own power and land safely being constructed in 1857. With the rise of the motor car industry and the availability of a light and reliable prime mover attention was directed to the possibility of controlled power flight.

The Wright brothers who had been carrying out extensive gliding experiments designed a motor and propeller to suit their machine, and in December 1903 were able to rise in the air and fly for a distance of twenty-five yards. This was the first flight of a machine fully controlled. Progress from then on was rapid, and successful machines were constructed in 1909 in England by Cody and by Roe. In the same year the world was startled by the first successful flight across the Channel by a Frenchman, M. Bleriot. Up till 1914 the building and flying of aeroplanes had been largely experimental, and little attempt had been made to put them to practical use, although experiments had been undertaken with seaplanes and other aircraft working in conjunction with the Royal Navy. With the advent of World War I, technicians, factories and Government funds were made available for urgent development work and a huge British aircraft industry was built up in an incredibly short time. Between 1914 and 1918 Great Britain manufactured over 50,000 aeroplanes, most of the engines for which were made in this country. After the war the reliability of the aeroplane was demonstrated by the flight across the Atlantic in 1919 of Sir Arthur Brown and Sir John Alcock. Soon afterwards regular air services were introduced between London and Paris. Between the two world wars much research and experimental work was undertaken, particularly by Air Commodore Sir Frank Whittle, on jet propulsion gas turbines for aircraft, and in May, 1941 the first aeroplane fitted with such an engine did its first flight. Tribute should also be given to the work of Dr. H. Roxbee Cox, Dr. A. A. Griffith and Mr. H. Constant and others at the Royal Aircraft Establishment, Farnborough, for their work in this connection. During the second world war further progress was made in the manufacture of large aircraft and the knowledge gained in their operation formed the basis of design of the present day machines used for long distance passenger service.

Probably the first working gas turbine was constructed by Armengaud and Lemale in Paris in 1904, and a second machine was constructed by Brown Boveri & Co. in 1906. Thereafter gas turbines were used merely as auxiliaries or as a convenient source of obtaining power from otherwise waste energy and not as serious competitors for power generation. Serious consideration was again given to the subject about the year 1935, when many steam turbine manufacturers began investigations based on the knowledge then available. The war for a time slowed up these developments, but as a result of the progress made in the use of gas turbines for aircraft, manufacturers both in this country and abroad turned their attention once again to the possibility of building successful industrial gas turbines. A number of firms have built experimental gas turbine units from which important data have been obtained. On the continent gas turbines for outputs up to 27,000 kW. are in operation, while in this country

machines for outputs up to 15,000 kW. are nearing completion. During 1950 gas turbines were used as prime movers to drive a motor car, a motor launch and locomotives from which valuable information has been obtained.

Reference has already been made to the development of the telegraph and telephone, but as these were dependent upon the use of a wire they had the disadvantage of not being able to communicate with ships at sea. The first idea of wireless communication was suggested by James Bowman Lindsay, of Dundee, in 1845, who proposed to use the earth or sea as a conductor, but on account of the very weak electric currents involved and the lack of sufficiently sensitive apparatus to detect them, this means of communication made little progress. During the latter part of the nineteenth century several British scientists conducted experimental research relating to radio-telegraphy, notably Clerk Maxwell, Sir Oliver Lodge, Professor D. E. Hughes and Admiral Sir Henry Jackson. It is, however, to Senator Marconi that we are indebted for his inventions in 1895 which made long distance wireless telegraphy possible. He conducted his original experiments in Italy, and in 1896 came to England. By the year 1899 he had established wireless communication between England and France, and two years later had bridged the Atlantic. A regular transatlantic telegraph service was established during the next seven years.

Following on the success of the wireless telegraph, experiments were commenced almost immediately on the possibility of radio telephony, and the most important inventions relating to this subject were perhaps the thermionic valve invented by Dr. J. A. Fleming in 1904, and the three-electrode tube invented by Dr. Lee de Forest in 1906. During 1914–1918 radio-telephony was used widely by the fighting forces, and valve techniques were developed rapidly to enable communication to be made with aircraft. By 1919 important experiments had been conducted by the Marconi Company and telephonic speech was established between U.S.A. and Paris and between Ireland and Canada. In the same year experimental broadcasting transmissions were commenced in England which culminated in the forming of the British Broadcasting Company, in October, 1922, and the setting up of regional stations.

The first details of a method to transmit actual images was disclosed in 1880 by Carey, an American, and although other inventors brought out ideas it was not until 1926 that the most successful proposal was described by John L. Baird. Baird used a scanning disk which carried two spirals of lenses, and the reflected light from the illuminated object was caused to affect a photo-electric cell. In 1929 the B.B.C. and the Baird Television Co. commenced a public television broadcasting service from Alexandra Palace, London, but owing to the intervention of the war little further progress was made. Since 1945 rapid advances in the technique have enabled new stations to be erected, and it is expected that television broadcasting programmes will be made available throughout the country by 1952.

During recent years considerable attention has been directed to the work of the scientists investigating nuclear energy, and to the possibilities of adapting this new source of energy to commercial work. The beginning of this era goes back to the year 1789, when a German scientist, Martin Heinrich Klaproth, discovered a substance which he named Uranium. Over 100 years had to elapse before its peculiar properties were observed by Wilhelm Roentgen, who discovered X rays, but it was not till 1896 that Becquerel identified the phenomena as radio-activity. The release of this information aroused interest in other scientists, notably Prof. Curie and his wife Marie Curie, and G. C. Schmidt. These scientists working independently discovered radioactive properties in thorium, polonium and radium, while Debierne discovered, in 1899, actinium. Chiefly because of the work done by Lord Rutherford, we know that these substances are spontaneously disintegrating into a series of radioactive substances, each of which is of lower atomic weight than its parent element. Many other scientists contributed to our knowledge of radio-activity, but it is to Lord Rutherford we are indebted for much of the pioneer work leading to the release of nuclear energy.

In 1942 the first atomic pile or nuclear reactor was put into operation in America and the first man-controlled release of atomic energy was achieved. During the war considerable progress was made due to the concentration of scientific personnel, and the spending of large sums of money which in peace time would have seemed enormous. Since 1945 Atomic Energy Research Establishments have been put into operation in this country ; at Harwell under the direction of Sir John Cockcroft, and at Sellafield under Sir Christopher Hinton.

And now 1951. We have seen the evolution of the steam turbine and its use on land and sea ; the turbo-alternator for the generation of electricity ; the distribution of electricity to factories and homes, and the manufacture of the thousands of miscellaneous electrical appliances ; the internal combustion engine and its use on land, sea and in the air ; the telegraph service ; the telephone ; wireless broadcasting ; television ; the gas turbine ; the motor car ; the aeroplane and finally nuclear energy. But in addition to these outstanding developments there have been many thousands of inventions relating to almost every branch of industry such as machine tools ; the typewriter ; calculating machines ; the gramophone ; the bicycle ; and the mechanisation of coal mining and agriculture to name but a few as examples.

The future

An address of this nature would not be complete without some prediction of the possible outcome of developments which, at the time of writing, are in their infancy.

Transport will always be a major item in civilised communities and there seems little doubt that the tendency in the future will be towards large scale railway electrification in those countries where population and traffic density conditions permit this being carried out economically. This tendency may well be hastened by the application of nuclear energy, on a large scale, to the production of electrical energy.

The production of electrical energy from fissile material is, even in the light of our limited experience to-day, a practical possibility. With the prevailing costs of coal and petroleum and the present-day costs of uranium or other fissile materials, it is doubtful whether electricity could be produced, at the moment, more economically by the last mentioned process than by other well established methods ; yet it requires but a small improvement in the method of using the heat generated by fission to enable atomic energy to compete with present-day practice. To justify the high initial cost of an atomic energy plant the production of electrical energy by fission must be carried out on a large scale, and so electrification of railways and other large scale usage of electricity will automatically result.

For a long time now I have advocated the rational usage of our greatest national asset—coal. It is unlikely that in the future our available supplies of coal will increase ; on the contrary they are likely to become less year by year. Economics will therefore force us to use coal more economically and I foresee high pressure gas mains supplying heat to home or industry, the complete abolition of the open coal fire, the increased use of slow combustion heating in homes using coke or other residual fuels, gas turbines living up to their names and using gas, and the use of coke, gas or tar, and of course, high ash coal residues for generating electricity in central power stations.

A great deal has already been done in the blending of coking and non-coking coals for gas production and I expect work of this nature will be intensified so that the total output of coke will be increased and valuable by-products obtained.

Work is now being undertaken in this country in burning methane, which occurs in small proportions in the large volumes of air used in mine ventilation and it may be that in the future, by using a high degree of pre-heat, prior to combustion, the miner's greatest enemy—firedamp, will be used for the benefit of man.

Without any doubt, in my opinion, the jet or gas turbine cum jet will be used to the exclusion of all other means of propulsion for all types of aircraft. This type of prime mover meets the requirements so admirably that it is difficult to see how any other form of power unit can compete. On land, the gas turbine is more sorely

pressed by its competitors and I doubt if any prime mover other than the steam turbine, will be used for very large powers. I do think, however, that the gas turbine will, to a great extent, supplant the diesel engine. There is no difficulty in making gas turbine units of 100 H.P. which could easily be fitted to motor cars within the space now occupied by petrol or diesel engines. The major remaining problem requiring solution to enable the gas turbine to take its place in our everyday life is that of a compact, low cost and highly efficient regenerative heat exchanger. The consideration given to this problem during our present meetings is an indication of its importance and likely solution. Once a purely rotating prime mover has been found suitable for a given application, it always becomes possible to supplant an alternative reciprocating type, and there is little doubt but that within the next fifty years, the gas turbine driven motor car will be used to the exclusion of all others.

Man's standards of living and comfort have increased only as he has made mechanical power his aide and servant in increasing degree. Electricity provides the most convenient way of supplying that mechanical power and if the nations of the world are listed in the order of electricity consumption per head of population it will be found to coincide with the nations listed in the order of living standards. Incidentally, nations listed in the order of protein percentage in their daily diet gives again the same sequence. Inevitably as nation after nation strives for improved standards of living and comfort, there must be a tremendous expansion in the generation and use of electricity. A consumption per head in Great Britain of five times our present electricity usage still would not produce demand saturation.

Large scale use of electricity for pump produced rain and for soil heating as a means of combating in some degree the vagaries of weather and thus help solve our evergrowing food shortages is certain to come.

Supersonic frequency electronically produced vibrations will become an everyday thing in our industrial, domestic and medical life. The thermionic valve will become an increasingly integral part of our life between 1951 and 2051.

When one considers the progress in engineering and the contribution to living standards in consequence which the century 1851–1951 has seen, he would be a brave—or a foolish man who would put a limit to prophecies for the future.

THE STUDY OF EARLY CELTIC METALWORK IN BRITAIN

Address by

SIR CYRIL FOX

PRESIDENT OF SECTION H

IN 1943 a great collection of Celtic metalwork from Llyn Cerrig, Anglesey, was spread out on a work-table in the National Museum of Wales, and I began to study the character, and the connections, of the decorated pieces in it (1).

In ordinary times this would have involved visits to the British Museum, and to other museums containing Celtic collections, but I was then of course restricted to a survey of the literature. While thus occupied, I was brought to realise how little of the mass of material had been published adequately, and how much had not been published at all.

Two classes of objects, mostly of bronze, are involved. There are 'works of art,' the finest achievements of Celtic craftsmen in Britain between *c.* 200 B.C. and A.D. 100, some of which are as familiar to students of art as to archaeologists: functional things elaborately decorated such as the Witham shield, the Thames (horned) helmet, the Desborough mirror, the Trawsfynydd tankard and the Torrs chamfrain. The group includes remarkable studies of living creatures: the ramshead handle from Harpenden, one of the boar-crests from Hounslow, and the oxhead finials from Lords Bridge, Cambs.

There are of course accomplished works in lighter vein, such as the bull's head from Ham Hill, Somerset, which very justly reminds my children of that Iberian Ferdinand who loved to lie and smell the flowers; and the duck from Milber hillfort, Devon, swimming away, alert and expecting interference, with a griddle-cake in its bill (2).

Of most of these and others in their class a good photograph or drawing is available. But there is no monograph dealing exhaustively with any one of them, no range of photographs necessary to the appreciation of three-dimensional works, and technical studies of those that are built up of several parts are almost non-existent. No one in the 70 years since it was illustrated by Joseph Anderson seems to have looked at the Torrs chamfrain closely enough to discover, as Professor Stuart Piggott has now demonstrated, that the famous horns are not part of the original design (2a).

It is perhaps the lack of studies of 'Phaidon' standard of these works—which is what they deserve—that has contributed to the lack of intensive interest taken in them by Institutes and Schools of Art in this country. But we archaeologists have preparatory work to do before this interest can be expected. Too many of us in the past, confronted by a complex Celtic pattern, have been content to provide a good illustration of it, which is left to speak for itself. I can provide an example of the risk involved in such a course of inaction. The late Reginald A. Smith, to whom we are deeply indebted for his work on our period, published in *Archaeologia* in 1909 a valuable record of Celtic mirrors, which included a superb drawing in colour by Charles Pretorius, a draughtsman of the highest reputation, of the magnificent Birdlip mirror. All Smith says in the text about its chief ornament is that the back of the mirror-plate is 'practically covered with eccentric scrollwork with a filling of basket-work pattern '(3). Smith, then, never gave close attention to the design; nor, it may be, did anyone else until recent years. For if any student had looked *intently* at the drawing—the only good published

illustration of the object—before or between the wars, he would have noted a surprising feature. The design is one of fold-over symmetry ; but this is departed from at one point, for a little triangle-within-a-circle on one side has no corresponding figure on the other side. When I noticed this in 1945, I supposed the unique symbol to be the ' signature ' of the ' Master of the Birdlip Mirror ' ; I delayed mentioning this in print, however, until I could examine the mirror itself at Gloucester. When this was possible, I found that the second circle and triangle *were* present ; [1] the draughtsman had erred, and his work had not been checked (3a).

So much for the outstanding pieces ; we have now to consider the mass of competent craftsmanship, and good design often repetitive, particularly important in the large collections from single sources.

Two of the largest hoards are those from Edington on the Polden Hill in Somerset, and Stanwick near Richmond, Yorkshire, in the British Museum, the former of over 70, the latter of over 90 objects, both of the first century A.D.

Nineteen of the Polden Hill bronzes were illustrated in the original brief account of the find, in 1803 (4), and a few more have since been published. Here I want to draw attention to three ' harness-mounts '—the use of which generic term illustrates the poverty of our nomenclature and the lack of interest in the technical aspects of our artistic heritage. These three bronzes are highly decorative ; two of them indeed are famous, coloured representations having, for many years, been sold as postcards at the Museum. They show on their backs ' projections or staples for fastening these pieces to their proper places ' as the writer of the original account vaguely but correctly remarks. These are of two kinds : flat angular bronze loops for straps, which need no comment, and hinge-and-catch fittings. In two of the mounts there is iron-rust in the hinge, and these fittings were undoubtedly for brooch-pins.

The only conclusion that occurred to me is that there were at times horsecloths hung on the ponies' backs to prevent them catching cold, or, in view of the unsuitability for war of these elaborate ornaments, for fixing coloured cloths for parade purposes. There is good classical authority for the use of highly coloured or patterned materials by the Gauls, and the technique would have come with the tribes to Britain.

The backs of these ornaments, with careful drawings of the ' projections ' were illustrated in 1803, but when in 1949 I noticed their significance, neither the member of the British Museum staff present nor I, could recall any later reference to, or explanation of them in the literature.

The Stanwick bronzes were unearthed in 1844 ; twenty of the objects were published in 1846, and again in 1906, and nine more in the second issue of the British Museum Guide to the Early Iron Age (5). No assessment of the whole of this remarkable collection is in print ; but Mr. Thurlow Leeds pointed out in his ' Celtic Ornament ' that there are portions of no less than four sets of horse-harness and chariot fittings, one of which he illustrated in a composite photograph (6).

Stirred by this illuminating comment, I recently, by courtesy of the Assistant Keeper in charge of the Sub-Department, followed in Mr. Leeds' steps, examined all the pieces and acquired some photographs. The first shows one circular harness-mount of each of three sets, in typological succession. I venture to emphasise to younger men and women who may be listening to me the fascination of the study of the interplay of tradition and invention which went on at the Celtic craftsman's bench. Such a sequence shows how ruthless, how insistent, in any Age, is the customer's demand for novelty. However good a design may be ' if it's ten years old it's a washout ' to repeat a colloquialism I overheard recently in a shop. It is apposite : for the series might easily be twentieth-century. The first piece, Edwardian baroque. Next, a simplified pattern —1920-ish—produced between the wars ; it avoids the lush curves of the older style. The last specimen is an unemotional presentation—note the ' new look,' the flatness : post-war stuff, obviously. But let us study the piece a little more carefully ; the craftsman is using a pre-war mould, for it has the same half-circle section as the

[1] The mirror was cleaned after the war, and a good photograph rendered possible, on the initiative of Mr. Charles Green, then Keeper of the Gloucester Museum.

others ; he has but turned the casting over, and tooled up the flat back.[1] The customer's whimsy must be met, but good material should not be wasted.

The Brigantian craftsmen, developing their ' lip ' and scroll motifs, made use of these in surprising ways. The miniature horse's mask—possibly decorating a chariot —was one of the too-few works of early art to be reproduced in Saxl and Wittkower's important *British Art and the Mediterranean*, published in 1948. Using lyre-shaped scrolls for the bony structure of the face and the nostrils, and the ' lip ' motif for the eyes, the bronze-smith has marvellously given life to abstract forms ; never was bland, smug, full-fed stable horsiness better delineated.

The very remarkable pair of human masks from the same source, though they show no such insight into character, also illustrate the range covered (as we must suppose) by ordinary craftsmen, for the scrolled beards and the eyelids can both be matched in patterns of their period. With eyes of coloured glass or enamel and golden pupils formed by the bronze pin-heads, they were striking—even fearsome —objects ; ornamenting (and protecting ?) a chariot or a dwelling. One of these, figured in the Museum's Early Iron Age Guide of 25 years ago, was pronounced a ' failure ' (7) !

[1] Students of the period will recall that the Brigantes were fighting at times not inappropriate to this sequence.

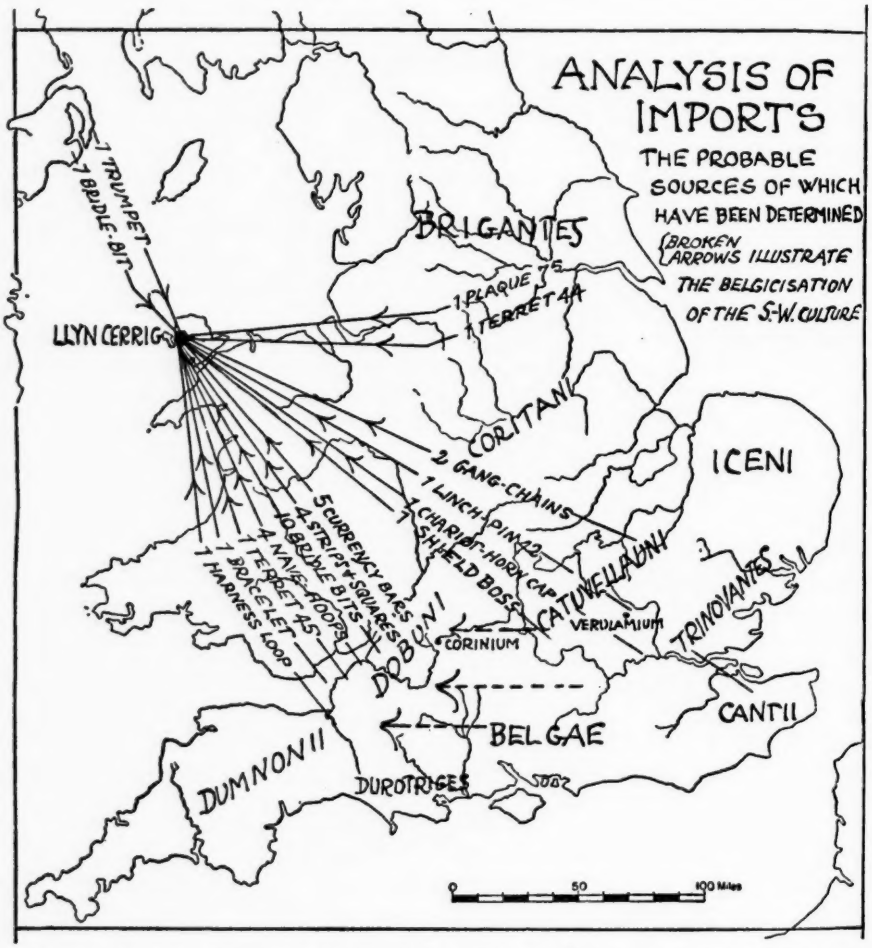


Fig. 1.—Analysis of Imports, Llyn Cerrig : *A Find of the Early Iron Age from Llyn Cerrig, Anglesey*, National Museum of Wales, 1946, p. 62.

It is a matter for surprise that considering the opportunity for monographs provided by these two partially published collections, and the well-merited popularity of Mr. Leeds' book, that no University teacher induced a student to take up the study of either, between the wars. The reason may in part be the amount of research work involved. The 'map of imports' which accompanied the Llyn Cerrig Report (8) and which is here reproduced (Fig. 1), is a convenient illustration of one aspect of the comprehensive synthesis needed. Since the survey of this find was completed, moreover, I have felt that there is no possibility of historically satisfactory treatment of important hoards or collections of the Iron Age without field-work—making personal acquaintance with the geography of the district—or without comparative study of all previous finds of metal-work in the region, to gain clues as to the likelihood of local manufacture.

The capacity of the Celtic metal worker to combine decorative craftsmanship and creative art as at Stanwick, is brilliantly shown in two of the four pieces surviving of a set of fittings for a chariot in the south of England—Bulbury hill-fort in Dorset (9). These two are based on the ox, with bodies moulded in smooth sweeping planes and with broad-based horn cones; the fantasy controlling the representation reaches a climax in the tail-tips, which are transformed into flowers. Each little beast rode, it may be supposed, on the rounded crests of a wooden yoke of a great lady's chariot, used for social calls.

The lack of appreciation shown by Mr. Leeds for minor art such as this, is to me a sad blot on his *Celtic Ornament*. We need, I suggest, in our studies more scholars who, like Dr. T. D. Kendrick, appreciate that such works are not to be judged by the extent to which they attain, or fail to attain, the representational competence of the Graeco-Roman world (10).

'If we go so far as to admit to some neglect of this aspect of Celtic studies by archaeologists,' you will say, 'what is to be done about it?'

Something will in time be done, on a wide front which will go a long way to bridge the gap between what is, and what should be. I refer to Dr. Jacobsthal's eagerly-awaited study of 'Celtic Art in Britain,' which we understand will be on similar lines to his *Early Celtic Art*, dealing with the continental material, published in 1944. But there will still be room and need, for such monographs as I have referred to; also, I venture to think, for studies on the lines I have myself been exploring for several years, and which require for their further development, if repetitive labour by each individual student is to be avoided, a card index of all the Celtic metal-work in Britain, centrally stored and readily accessible to all.

I deal with these matters in succession. The procedure defined for me in 1943 by circumstance, was to analyse a design of the finest quality—the Llyn Cerrig, Anglesey, plaque, to search for the sources of its elements and structural form and to see what influence the design had on later metal-work. To trace, in short, the life-history of an artistic idea. This led, unexpectedly, to an understanding of the Birdlip and Desborough mirror patterns, and to a demonstration of the interest of almost unknown, or neglected, works.

The plaque, a work of the first century B.C., is a disc of thin bronze with a large circular hole near its upper margin, and a strange design in relief occupying its lower part. The central asymmetric figure, a triskele, shown on Figure 2(a), is built up of domed trumpets with bosses in terminal coils, supported by stem, and marginal, lobes—all, as it were, in rapid anti-clockwise movement; this figure is framed by larger lobes, inert, in two groups of three. The three voids are important; they are each bounded by two simple, and one compound, curves (10a).

Elements of the design are found in earlier British metal-work made for war-like display; the trumpet motif in the Torrs chamfrain and the Ulceby bit-link; the lobe pattern on a disc from a warrior's grave at Grimthorpe in Yorkshire (11).

The Llyn Cerrig relief pattern persists down to the end of Celtic freedom in south Britain in the first century A.D., and is met with in Romano-British work (12). Surviving examples are for the most part debased, but there is one of surprising quality and interest.

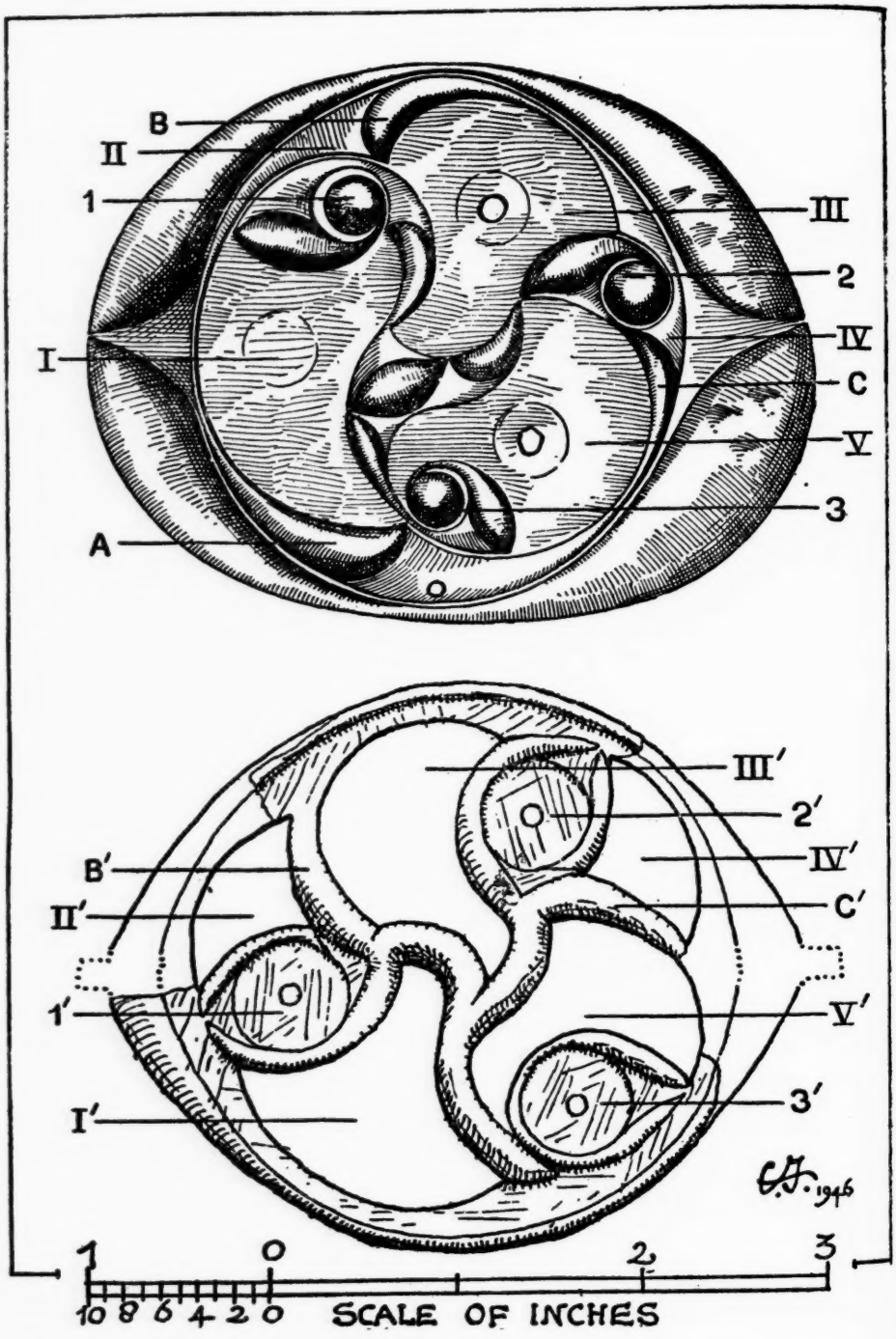


Fig. 2.—(a) Part of relief on a bronze plaque, No. 75, from Llyn Cerrig, Anglesey :
(b) Openwork disc in the Ashmolean Museum, Oxford. 1947, *Antiq. Journ.*, XXVII, p. 3.

This is an open-work disc in the Ashmolean Museum, of south British workmanship, probably Dobunic (13). It is the same on both faces : a triskelar figure, asymmetric, with the Llyn Cerrig bronze bosses replaced by applied settings, now lost. The essential structure (Fig. 2b), is a roundel, but the rim is expanded on either side, and it can only have been fixed by trunnions ; it would then have been turned over and over by wind and movement. It most probably decorated a chariot-pony's mullen or headstall. Most striking, seen sideways, would have been the conjoined half-globes of bright colour in their bronze bands. This is not an imaginative notion ; the well-known Datchet brooch from the River Thames illustrates the technique, in orange glass (14).

If the restored disc is studied side by side with the plaque, the relationship is evident, but the abstract threesomeness of the latter has become naturalistic, a tree being represented, with one of the coloured globes lying free beside the thickened trunk. In the published account I have suggested that we may perhaps regard this dainty object as illustrating a widespread myth familiar to the Greeks, of the golden apples in the Garden of the Hesperides. The date of the disc should be early first century A.D.

The three-dimensional structure of the plaque was translated into a two-dimensional pattern soon after its creation ; a striking example of this is provided by a shield-boss in the same collection from Anglesey (15). It is a handsome piece, with the pattern in pairs on either side of the central rib. This pattern has a quality new to us, tenseness : an expression in abstract art of force held in leash. The movement is clockwise, but this is countered by a limb curved in the opposite direction and held by the line which bounds each figure.

The quality I refer to is little recognised n Celtic patterns ; it may then be worth while to refer to the open-work design on a scabbard mount found with other first century A.D. things on Lambay Island near Dublin (16). This has inner and outer triskeles, so shaped as to appear to be moving in opposite directions. As they have interlocking elements the design is balanced, immobile : but not restful.

This development, though interesting, is of secondary importance in the history of the triskelar pattern, for the design was taken over, in more faithful copy, by craftsmen engaged in the flourishing business of mirror-making for ladies of the British aristocracy. So far as we know, it was the first design to be incised on these beautiful objects in Britain, and I think that all the eleven mirror designs known to us are derived from it (17). On one mirror there is a two-roundel design, on eight a three-roundel ; two others are held to represent a disintegration of the latter pattern. This synthesis was illustrated diagrammatically in 1945, elements of the three-roundel design being grouped as ' isolated,' ' fused,' or centralised (18). The diagram is reproduced, slightly amended, on Figure 3.

That it was possible to reduce to such order the apparent chaos of the eleven patterns indicates the extent to which the Celtic bronze-worker was controlled by precedent and convention, as well as (having regard to the beauty and variety of some of the developed patterns) his originality within the limits thus set.

The descent of the (typologically) earliest of the incised patterns, those on the ' Mayer ' mirror, is readily understood when the Llyn Cerrig design is translated into a linear pattern and placed above them, and it is interesting to notice how far from the original the design can travel and yet be recognisable : the mirror from St. Keverne, Cornwall, illustrates this point (19).

The recognised high-lights of the mirror series are the Birdlip and Desborough mirrors : to which we must now add ' Mayer.' The brilliant device of ' linking and opening the roundels ' which this latter mirror displays, inspired and rendered possible subsequent developments shown in the former. But while the transition from Birdlip to Desborough is easy to follow, that from ' Mayer ' to Birdlip is not ; and it was a piece of good fortune that provided me in 1948, thanks to Mr. M. R. Hull's skilled help, with the ' bridge,' the Colchester mirror, the design on whose encrusted and fragmentary plate he was mainly instrumental in recovering. This design is an early product of the pre-conquest Romanisation of

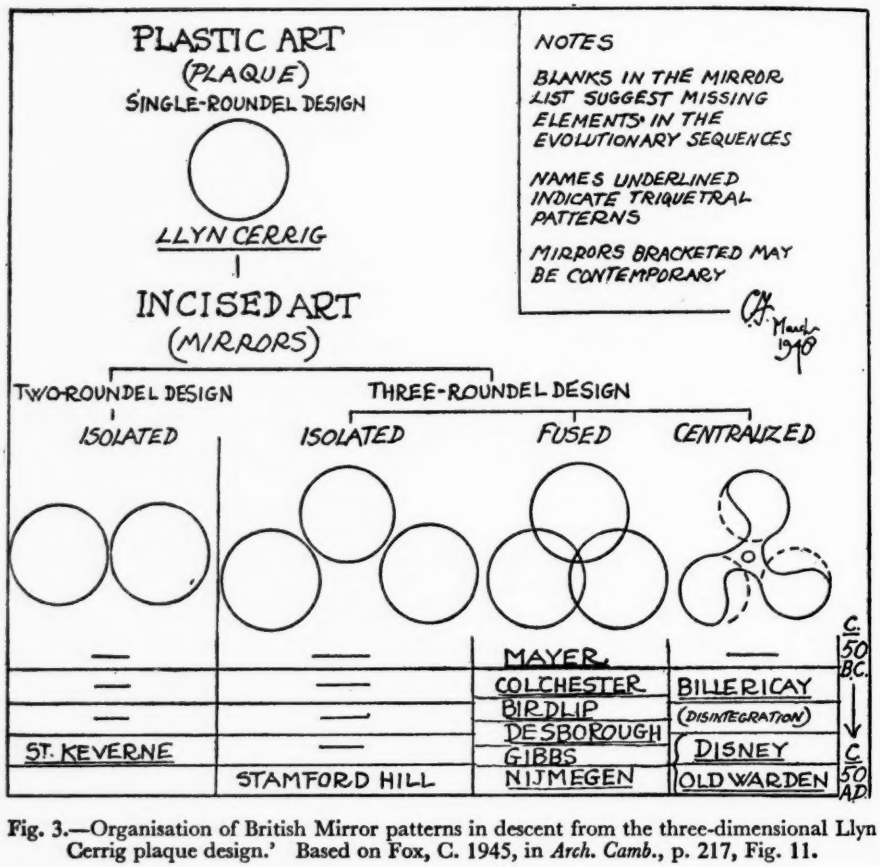


Fig. 3.—Organisation of British Mirror patterns in descent from the three-dimensional Llyn Cerrig plaque design.' Based on Fox, C. 1945, in *Arch. Camb.*, p. 217, Fig. 11.

southern British art, to which influence, as we have been taught to see by Mdlle. Françoise Henry, its fold-over symmetry is due (19a). The palmette and the lyre-scroll which the pattern show, emphasise its source. The main lines of the designs on the four mirrors were drawn in sequence, and the transmission of important features indicated, in a paper published in 1948 (20).

It is significant that the development of pattern on the mirror-plates is accompanied by development of the handles: Colchester, Birdlip and Desborough show a progressive complication of the simple ' gamma ' form of ' Mayer ' (21). There is first seen, in the simple form, a decorative addition to the upper portion of the handle where it grips the mirror-plate: (Stamford Hill mirror): then the handle is lengthened by the addition of a second loop or ring (' Gibbs ' mirror).

The final form—which shows several variants—is based on the doubled loop to which a ring is added; the Birdlip mirror has the most elaborate example of the type. The development is associated with a most ingenious and sophisticated effort at integration of mirror and handle. The C-sectioned binding of the thin mirror plate, and the upper ends of the handle, were regarded as a single unit and designed accordingly; the only question was where, in the interest of an effective slotting-in of the mirror plate, the cut should be made: where, that is, the solid casting should stop, and the hollow binding begin. The essence of the plan was to create illusion: to make the actual riveted joint (of binding and casting) invisible. This was accomplished in both the Birdlip and Desborough mirrors. So completely masked were the two riveted joints of Desborough that in this formal pattern, distinguished for its fold-over symmetry, the bronze-smiths were

able to make the *cast* bronze arms of the handle of strikingly unequal length, confident that this would never be noticed !

I think we might go further in an endeavour to understand how these elaborate mirrors with fold-over designs beginning with the Colchester mirror and culminating in Desborough came to be wrought in the first 30 years or so of the first century A.D. than is implied in a vague reference to Roman influence. In their Celtic fashion, and highly specialised idiom, these mirrors reveal something of the delicacy of perception and execution, the serenity of mood, which characterises Roman decorative art in the Augustan age, and the possibility of a link should be sought. The effect of this Imperial art movement on the Gallo-Belgic periphery of the Empire and beyond, need not, of course, be assumed to end with the death of Augustus in A.D. 14. The proposition does not lack an analogy, that of the effect on ancillary crafts of the Gothic tradition in Britain, of the first breath of the Italian Renaissance at the turn of the fifteenth and sixteenth centuries A.D.

I have particularly in mind the acanthus scrolls which decorated the outer wall of the *Ara Pacis Augustae*. As Mrs. Strong remarks : ' The scrolls became a typical Roman decoration ; they adorned furniture and small objects.' A network of such spirals, she remarks, ' encircles a cup from the Treasure of Hildesheim ' (22), near Hanover.

That portable works of art of this quality and character trickled into south-western and south-eastern Britain in the half century or so before the Claudian conquest is likely enough. Two decorated silver cups, probably from Campania, were in the Welwyn, Herts. burial group of this period, together with a pair of handles of an imported silver kylix (23).

To come down to detail, it is not easy to understand how the lace-like effect of the enrichment in the Desborough mirror scrolls could have developed *in vacuo*, but regarded as an attempt to create in the Celtic idiom the play of light and shade, the glitter of silver plate decorated in relief with festoons of *Ara Pacis* quality, it seems to me readily explicable.

Again, we are I think at a loss to explain the sheaths on the Birdlip handle without reference to the natural renderings of stem-sheaths on cup handles, such as that from the Bernay (Eure, France), treasure (24).

The mirror distribution pattern is significant in this connection. Mirror finds tend to be concentrated in or near the ports of the south-east and the south-west of the island. It is not that any of our mirrors were imported—the type with decorated plates is unknown in Gaul, and the ' Etruscan ' mirror type had long been forgotten in Italy. It surely was that the women in the higher ranks of British society who were sophisticated enough to commission these costly things from the court craftsmen, were those living in areas, and under rulers, Catuvellaunian or Dobunic, most open to the influence of Roman manners and customs, as shown by their inscribed coins. In their turn, we may suppose, the craftsmen studied everything continental they could get hold of.

We have now to consider why the triskelar pattern with which we are concerned spread like wildfire over Britain, and why though it developed new aspects, its threesomeness persisted. It was most probably because this figure had a ritual or magical significance among the Celtic tribes : it was a protection against the evil eye, or mishaps generally—hence the Ashmolean disc swinging, actively functional, above the chariot pony's head.

This probability is reinforced by the discovery at Barnwood in Gloucestershire by Mrs. E. M. Clifford, of a small carved aniconic object of limestone which cannot have any use other than in this shadowy, magical field : it is covered with triskeles, four of which are formalised representations of the Llyn Cerrig pattern, and dates in the first century A.D. This very important piece for the study of Celtic beliefs and their relation to art has received little or no attention since it was published in 1937 (25).

To sum up : if then, study of the evolutionary progress of patterns, and of the motifs which are embodied in patterns in Celtic art in Britain be worth while, active and substantial encouragement should be given in Universities to students qualified to carry out postgraduate research on such themes. I have followed one shining thread of Celtic achievement, a kind of

pursuit in which Reginald Smith, Mr. Thurlow Leeds and many others preceded me ; nevertheless, I daresay there are dozens of related phenomena in the mass of material available for study, offering rich reward in the understanding and appreciation of the subtle intelligences and skilful hands, active in this country two thousand years ago. No difficulty will be experienced in Scotland : Professor Stuart Piggott is working on the Torrs chamfrain and other fine pieces with the co-operation of Mr. R. B. K. Stevenson, keeper of the National Museum of Antiquities, and is inspiring enthusiasm in his students.

As for the card index : all decorated examples of Celtic metal-work in particular should be recorded, in private hands so far as is possible as well as in museums. Every such piece should be drawn, unless a good illustration is available in a reasonably accessible publication. By ' good ' I mean one which reveals the detail, as well as giving the general effect.

When a card index is mentioned, older members of the Association will at once recall the honoured name of Harold Peake who, aided by an annual grant generously provided by this Association, prepared an Index of Bronze Implements, with a measured outline drawing on each card. This activity stimulated interest in a great and important group of antiquities, assisted in the preservation of the provenance of many examples in small (and sometimes neglected) museums, and enabled distributional and typological studies to be carried out with comparative ease, studies which rapidly enriched our knowledge of the Bronze Age and its techniques.

The Index had one defect. It was not associated with an institution—though the British Museum has now kindly given it a home—and so has not been automatically maintained as a living entity by the inclusion of new material as it comes along. This is essential, of course, for the usefulness of such an Index as a tool of research. I hope then that this suggestion of mine, for an Index of Celtic Art, will appeal to a university, or to one or other of the institutions in this country which could fittingly organise and maintain it. A record intended to be complete rather than selective is essential. Students, by using such an index intelligently, should learn from it what museums to visit, what illustrated articles to read, for any type of design, and any individual motif used in Britain. The ' trumpet ' sequence figured in my Llyn Cerrig Report shows one aspect of what would be obtainable ; it is I am sure, very incomplete. A school of research on Celtic Art might easily come into being around the effort I have in mind ; and I hope such a scheme would be regarded as worthy of consideration by the Courtauld Institute itself.

In conclusion, we must always have in mind, in studying elaborate Celtic patterns, the possibility that the human face is also represented—shadowed forth, as it were. Professor Christopher Hawkes, at a recent Oxford gathering, suggested that human eyes, nose and mouth were intentionally present in the patterned symmetry of the Colchester mirror, and I have since seen the like in the Birdlip pattern. The student will have in mind the likelihood that such insular simplicities, if they exist, would stem from a continental achievement, that astonishing series of faces illustrated by Dr. Jacobsthal in *Early Celtic Art*, and so vividly described in his earlier Rhys lecture to the British Academy, *Imagery in Celtic Art*. The Celts ' *see* the faces *into* the spirals and tendrils; ambiguity is a characteristic of Celtic Art ' (26).

Human faces apart, one can easily get lost in the intricacies of some of the Celtic patterns :—sometimes voids, sometimes solids in a pierced open design seem dominant ; sometimes the positive, sometimes the negative elements in relief or incised work seem to be in control ; at other times no unity in a pattern can be appreciated without using both elements. It is, in truth, often very difficult to find secure anchorage for one's mind or sensibilities in contemplation of such creations. I realise, then, how hazardous it is for me to speak or write about my incursions into that intensely interesting, but so often alien and twilit world of Celtic imaginings embodied in Celtic metal-work.

No one at a Section H meeting will be surprised when I say that during the preparation of this Address I have had maxims by Dr. A. C. Haddon in mind (27). One,

on which my principal theme is based, and which is wholly acceptable is 'Similar designs, taken from a limited area, very probably have a genetic connection.' The other is, 'Barbaric art is easier to study than the complex art of civilised peoples.' I wonder if, in the period we are studying, that of skilled barbarian craftsmen in contact with a high civilisation—contact close enough to be stimulating, and not so close as to be overwhelming—this is necessarily true.

REFERENCES

(1) *A Find of the Early Iron Age from Llyn Cerrig, Anglesey.* National Museum of Wales, 1946, pp. 46–58.

(2) The following is a select list of illustrations of the objects mentioned :—
R. Witham, Lincs., shield, *Early Iron Age Guide, British Museum*, 2nd. ed. Figs. 113–115 ; Kendrick, T. D., in *Anglo-Saxon Art*, Pl. I ; R. Thames, helmet, *E.I.A. Guide, B.M.*, 2nd. ed., Fig. 116 ; Desborough, Northants., mirror, *Archaeologia* 61, Pl. XVIII, p. 338 ; Trawsfynydd, Merioneth, tankard, *Guide to Prehistoric Collection, National Museum of Wales*, 1939, Pl. VIII ; Torrs, Kirkcudbrightshire, chamfrain, Anderson, J., *Scotland in Pagan Times, Iron Age*, Figs. 92–93 ; Harpenden, Herts., ram's head, Leeds, E. T., *Celtic Ornament*, Fig. 28, p. 93 ; Hounslow, Middlesex, boar, Kendrick, T. D., in *Anglo-Saxon Art*, Pl. II,1 ; Lords Bridge, Cambs., ox heads, Fox, C., in *Antiq. Journ. VI*, 316, Figs. 1–3 ; Ham Hill, Somerset, bull's head, *Proc. Soc. Ant.* XXI, 1906, p. 133 ; Milber hill fort, Newton Abbot, Devon, duck, Cottrill, F., Interim report, *Devon Arch. Expl. Soc. 1937.*

(2a) Since this address was prepared, a *Picture Book of Ancient British Art*, has been prepared by Professor Piggott and Dr. Glyn E. Daniel, including 36 large size illustrations of Celtic pieces with a commentary.

(3) *Archaeologia*, LXI, Pl. XLII, pp. 330, 332 ; *E.I.A. Guide, B.M.* p. 122. A re-examination reveals the faint pencilled outline.

(3a) *Antiq. Journ.* 1948, p. 133 n.

(4) *Archaeologia*, XIV, p. 90 ff.

(5) *Archaeologia*, LX, pp. 289–290 ; *E.I.A. Guide, B.M.* p. 139 ff, Figs. 151–161.

(6) Leeds, *Celtic Ornament*, Fig. 31 and pp. 102, 109, 121.

(7) *E.I.A. Guide, B.M.*, Fig. 158, p. 141.

(8) *A Find of the Early Iron Age from Llyn Cerrig*, Fig. 34.

(9) *Archaeologia*, XLVIII, 1845, pp. 115–120, Pl. VI.

(10) *Celtic Ornament*, pp. 94–95 ; 1938, *Anglo-Saxon Art*, pp. 6–7. There is a good illustration of the boar on Pl. II, 1, of the latter work : of the Belbury creatures in *Archaeologia*, above.

(10a) Fox, C. *Llyn Cerrig*, Pls. I and XIII.

(11) Fox, C. *op. cit.*, Figs. 24, 26.

(12) Wheeler, R. E. M. and T. V. 1936, *Verulamium*, Soc. Ant. Lond., p, 216 and Fig. 48. Leeds, E. T. *op. cit.*, Fig. 36a and p. 139 ff.

(13) Fox, C. 'An open work Bronze Disc in the Ashmolean] Museum.' 1947, *Ant. Journ.*, pp. 1–6.

(14) *Proc. Soc. Ant.*, 2nd Series, XV, 1893, opp. p. 191.

(15) Fox, C. 'A shield-boss of the Early Iron Age from Anglesey.' 1945, *Archaeologia Cambrensis*, pp. 199–220.

(16) Macalister, R. *Proc. Roy. Irish Acad.*, Section C, Vol. XXXVIII, Pl. 24.

(17) Fox, C. 1948, *Arch. Camb.*, Pls. I and II, p. 28.

(18) Fox, C. 1945, *Arch. Camb.*, p. 217, Fig. 11.

(19) Fox, C. 1945, *Arch. Camb.*, p. 207, Fig. 4.

(19a) An earlier symmetry, also of classical origin, had been lost.

(20) Fox, C. and Hull, M. R. 'The Incised Ornament on the Celtic Mirror from Colchester.' 1948, *Antiq. Journ.*, pp. 123–137, esp. Fig. 7.

(21) Fox, C. 'Celtic mirror handles in Britain.' 1948, *Arch. Camb.*, pp. 30, 33.

(22) Strong, Mrs. E. 'The Art of the Augustan Age.' *Camb. Anc. Hist.*, X, pp. 549–550 ; *Das Hildesheimer Silberfund.* Pernice, E. and Winter, F. Berlin, 1901, esp. Pls. VI and VII.

(23) *Archaeologia*, LXIII, pp. 20–21, Fig. 20 and Pl. II.

(24) In the Louvre. See Babelon, E., *Le Trésor d'Argenterie de Berthonville près Bernay* : Paris 1916.

(25) *Antiq. Journ.*, XIV, p. 60, Figs. 1–4.

(26) *Proc. Brit. Acad.*, XXVII, 1941, p. 301 ff. esp. p. 308.

(27) *Evolution in Art, as illustrated by the Life-histories of Designs*, 1895.

THE PHYSIOLOGY OF THE CAPILLARIES

Address by

PROF. H. P. GILDING

PRESIDENT OF SECTION I

WE are to-day to turn our thoughts to what promises to be an interesting and I hope fruitful discussion on Body Water. I make no apology for opening this session with an account of my own views on the transport of water around the body. Since Harvey's time a great deal of attention has been paid to the study of the circulation as a whole. Yet it must strike any serious student of physiology as odd that so little is understood about the *raison d'être* for the circulation, viz. the many and varied exchanges that must continuously go on if life is to be maintained.

Our understanding of the physiology of capillary circulation would be considerably helped were the knowledge of their structural relationships to be more widely disseminated. The physiologists of a century ago appreciated the importance of an accurate study of the structure of a tissue whose functions they were attempting to elucidate and were themselves originators and masters of techniques necessary for this purpose. Bowman and Todd in their 'Cyclopaedia of Anatomy and Physiology' describe the blood supply to muscles as follows :—

'The arteries and veins of muscles commonly run together, and most of the arterial branches, to within two removes from the capillaries, are accompanied by two venae comites. They invariably pass more or less across the direction of the fibres, divide and subdivide, first in the intervals between the larger sets, then between the smaller sets, till the ultimate twigs insinuate themselves between the fibres composing the smallest bundles, and break up into capillary terminations. In this course the vessels supply the areolar tissue, their own coats, and the attendant nerves. The capillary plexus of the areolar membrane consists of irregular but pretty equal-sized meshes, and contrasts strongly with that of the muscular tissue itself. The proper capillaries of muscle are quite characteristic in their arrangement, so that a person who has once seen them can never afterwards mistake them. They consist of longitudinal and transverse vessels, the longitudinal always following the course of the elementary fibres, and lying in the intervals between them, the transverse being short communications placed at nearly equal distances between the longitudinal ones, and crossing nearly or quite over or under the fibres. The manner in which the longirudinal vessels are placed in relation to the fibres, is seen in fig. 286, where I have represented them as they are seen on a transverse section. They usually occupy the interstice between three or more fibres, but sometimes also the space between the contiguous surfaces of two fibres. The length of the longitudinal vessels does not usually exceed the twentieth of an inch; in other words, the terminal twigs of the artery and vein pertaining to the same capillary are seldom further than that apart. The length of the transverse anastomosing capillaries necessarily varies with the thickness of the fibres over which they pass. The diameter of the capillaries of muscle varies like that of others with the size of the blood-particles of the animal. It is, however, only just sufficient to allow of the particles to pass. If a fragment of a frog's muscle, perfectly fresh, be examined, series of blood particles are compressed and elongated, sometimes to a great extent, evidently by the narrowness of the canal which contains them. It may seem at first sight not doubtful that in the living creature these elastic blood-discs are similarly elongated in their passage through the vessels of muscle, but the

admirable researches of POISEUILLE will perhaps serve to explain this appearance without our being driven to suppose the presence of so formidable an obstacle to the capillary circulation through these organs. It is more probable that the contraction of the vessels and the compression of the blood-discs occur, on some of the contents of the vessels being permitted to escape by the severing of the fragment for microscopic examination. The coats of the capillaries of muscle consist of a simple diaphanous membrane, in which a few irregular-shaped cytoblasts occur at infrequent intervals.'

This clear statement of the final arrangement of alternating arterioles and venules has received scant attention from those working in the capillary field. The arrangement is not peculiar to skeletal muscle for Bowman and Todd produced similar evidence for tissues so structurally different as the papillae of the tongue, the smooth muscle of the rectum, and the skin of the frog. In a lobular structure such as the liver the capillary distribution follows a physiologically similar pattern The arteriolar end of the sinusoid—if such we may call it—supplies a column of three or four cells in thickness between it and the next sinusoid. Whereas at the venous end only one liver cell separates the sinusoids. These findings can be readily demonstrated by perfusion with 1/1000 solution in saline of Janus Green after preliminary perfusion with Ringer's solution to remove blood. (Janus Green agglutinates in the presence of plasma and serum and plugs the vessels.) The excess dye is removed by further saline perfusion until the saline comes away clear. Permanent preparations can be mounted if the Janus Green is subsequently fixed by perfusing 5 per cent. Ammonium Molybdate. After Janus Green perfusion the nuclei of smooth muscle fibres and of endothelial cells stand out with great clarity. The elastic laminæ of arteries is also stained. Unfortunately this method does not work in structures such as the kidney, liver or skeletal muscle, for here the dye is converted into a red compound. The capillaries in these tissues are best seen after intra-arterial fixation with formalin followed by Hæmatoxylin. These simple techniques are no more difficult in execution than is the Carmin Gelatin injection so frequently used by histologists to demonstrate the distribution of the vascular system. This method gives a picture of the channels through which the blood flows, but gives no indication of the structural details of those channels. After intravascular staining with either Janus Green or Hæmatoxylin the nuclear detail of arteries, capillaries and veins is self-evident and presents the student with a picture that is readily appreciated and understood.

That the space between one arteriole and the next in muscle, as mentioned by Bowman, is of the order of 2/20ths of an inch has been amply proved by Rous and his colleagues. 'The groups of muscle capillaries which connect transverse arterioles with transverse venules vary in length from 0·43 mm. to 1·35 mm. in the adductor magnus, and average 0·69 mm. In the external oblique they are as long. Krogh ascertained that those which are open in resting guinea pig muscle have an average diameter of only 3·5 μ. Corpuscles are deformed while passing through them. In injected and fixed rabbit muscle they range between 2·5 and 5·5 μ. It follows that the capillary length is often several hundred times its breadth. The merest glance at an injected specimen poses the problem of tissue maintenance by such vessels. Blood coursing through the hairlike channels, running the gauntlet of protoplasm that both takes and gives, is inevitably so different on emerging from what it was on entering that the segment of muscle fibre served from the distal portion of the capillaries would exist in a totally different milieu from that at their beginning were not conditions equalised in some way.' What is not as yet revealed is the pattern of the arteriole–capillary–venous arrangement in the transverse direction of a structure like muscle. Judging from the work of the last-named authors this must be a considerable distance in histological terms, for the patterning in muscles seen after the injection of colloidal dyestuffs goes deep into the substance of the muscle. Whatever view is held about the permeability of the vessels, account must be taken of the distance substances must diffuse through a compact structure such as muscle.

The work of Zweifach has called attention to direct communicating vessels from arterioles to venules which he calls A-V bridges with a subsidiary system of true capillaries surrounding these miniature anastomoses. This arrangement is readily seen in a membranous structure such as the peritoneum. There is little evidence that it holds in more solid structures with a high metabolic rate. As arteries dichotomise to arterioles the elastic laminæ or fenestrated membrane becomes an increasingly loose network of elastic fibres until finally in the ultimate arteriole only a delicate externally placed spiral of elastic tissue remains. This spiral disappears before the capillaries arise from the arteriole. Similarly the circular and the thinner layer of longitudinal smooth muscle fibres become increasingly fewer until only one or two spirally placed muscle fibres remain. The true capillary begins at the point where the investment of connective tissue and muscle ceases and continues until these structures reappear at the venous end of the vessel. Speaking generally the venous ends of the capillaries are of wider diameter and the endothelium more attenuated than at the arterial ends. Nearly all capillaries anastomose with their neighbours and it is rare to find a capillary continuing a solitary course from arteriole to venule without communicating with a similar vessel. These 'diaphanous tubes' made of thin endothelial cells neatly cemented together have outside their walls scattered irregular shaped cells called cytoblasts (by Bowman), Rouget cells, pericytes, or adventitial cells which have been of considerable interest to physiologists for the past hundred years. If the macrophage system is stained intravitally and the tissues subsequently examined one is forced to the conclusion that the Rouget cell is nothing more nor less than a macrophage sessile on the vessel wall.

There is ample evidence to confirm Beale's original observations, made 90 years ago, that a fine plexus of non-medulated nerve fibres surround the capillaries. Remak's bodies or nuclei are found at some of the transections of this plexus. There is no indication of intracytoplasmic nerve endings in the endothelial cells. It is tempting to think that Ramak's ganglia are the liberators of the chemical mediators of the nervous impulses.

Much labour has been expended on the question of capillary contractility and that the results of these efforts have so often been conflicting should occasion no surprise. The varying techniques used have only too frequently led to the examination of the vessels in an entirely abnormal environment, e.g. the exposure of tissues to adequate illumination for microscopical observation in itself produces an almost paralytic dilatation of these small vessels let alone the reactions invoked by the necessary operative interference. Factors such as these have received little mention in the literature. In spite of these criticisms we can say positively, however, that capillaries are contractile and what is more they can contract against relatively high pressures. It is only necessary to quote two instances in support of this ; if capillary dilatation be produced by venous congestion so that venous pressure rises to 60–70 mm. Hg, it is possible to produce capillary constriction through either the local application of adrenalin or else the elicitation of the 'white' reaction of Lewis. When the circulation is arrested after the preliminary production of venous congestion Bier's spots develop after a lapse of about ten minutes, if now the blood be expressed from the hand by tight bandaging with rubber and the pressure in the arterial occlusion cuff be raised to 200 mm. Hg not only do the Bier's spots in the forearm remain but further Bier's spots will develop. This drastic treatment produces pressures sufficiently great to bring about petechial hæmorrhages here and there in the skin of the arm, but in spite of this, capillary contraction takes place as evidenced by the Bier's spots. The negative aspect of this problem is seen when testing for capillary fragility in vitamin C (or is it P?) deficiency experiments. This fortunate property of the capillaries is vitally necessary in the circulatory stresses that the body is heir to. If, as some have supposed, these small vessels depend on the pressure of their contained blood for their patency then active readjustments in the vascular system would be slow and difficult of achievement.

The means by which contraction occurs is still a debatable point. The view that the Rouget cells are the contractile elements which was so popular thirty years ago has now little support. Indeed a re-examination of the evidence for this view makes one wonder why it was ever accepted. Capillary contraction has been observed over and over again by many different workers at sites where no Rouget cells were present, so even had the Rouget cell been a contractile element, evidence should have been offered to explain contractions occurring elsewhere. The first tentative explanation of capillary contractility that I can find is that put forward by Beale in 1872. 'The smallest veins, like the capillaries, are destitute of muscular walls, but are remarkable for the great number of oval bioplasts connected with their coats and projecting into the interior of the vessel. In many of the smaller veins the united area of the bioplasts would considerably exceed that of the membranous portion of the wall of the vein. These bioplasts are the agents concerned in the absorption of nutrient constituents from the blood and their distribution in an altered form to the tissues. It is these bodies which, by removing and taking up certain of the substances dissolved in it, promote the onward flow of the blood. Thus the *vis-à-fronte* action, which has been so frequently referred to, may be accounted for and explained.' His assumption called attention to cells which were probably the cells termed 'irregular shaped cytoblasts' by Bowman and later came to be called Rouget cells. The means by which these cells 'imbibed water' was not discussed but Beale did state that it could be produced by stimulation of the nerves supplying the frog's web observed. Some observers record longitudinal folding of the endothelium as contraction takes place; if such occurs then it would seem that extraneous factors such as Rouget cells or tissue fluid pressure must be the cause. Frequent mention has been made in the last twenty years of endothelial nuclear swelling. The beautiful ciné film in colour made by Florey and his colleagues makes a convincing demonstration of this. An examination of this film leaves no doubt in the observer's mind that the endothelium swells on stimulating the sympathetic and that this swelling is responsible for the capillary shut down.

Many references have been made to the spontaneous opening and closing of capillaries under observation. I have required my students to observe the capillaries in the nail bed for periods up to half an hour at a stretch and no single one of upward of an hundred students would admit having seen any vessel shut down or open up. Krogh's india ink experiments so widely reported and accepted would appear to be conclusive for structures such as muscle. On repeating these experiments, however, a real snag was revealed which casts some doubt on his observations. When india ink, prepared by dialysis with Ringer's fluid and subsequently centrifuged, is injected intravenously while tissues are under microscopic examination, it is immediately observed that the ink agglutinates in the blood stream and no longer remains a dispersion of carbon. Certain arterioles are plugged by particles while neighbouring vessels more widely dilated allow the particulate matter to pass. Judging by the presence of ink in the vessels post mortem one would say that the capillaries beyond the plugged arteriole were shut down while those where the ink had penetrated were open. I may say that of the several brands of india ink tried the one used by Krogh himself was one of the worst offenders in this respect. I mention this observation merely to show how extremely careful one must be in deducing physiological facts in a field which is not normally within the range of human vision or control.

That the capillaries are dilated and have an increased blood flow through them in active tissues is uncontested. The means by which these changes occur is even now not clear. Our textbooks have it that metabolites are responsible. The hydrogen ion concentration required to produce comparable changes in perfused tissues is at or below *p*H 5. The view that acid metabolites such as CO_2 or lactic acid are responsible for the vaso-dilation is therefore untenable. Sympathetic nerve stimulation brings about an intense vaso-constriction in muscle but if the motor nerves are simultaneously

stimulated the constriction is replaced by vaso-dilatation. This does not occur if the animal be curarised. It would seem, therefore, that in active tissues the increased O_2 utilisation brings about a local anoxia and this accompanied by the production of substances such as CO_2 plus the local rise of temperature may in part explain the dilatation that occurs. It is known that in reactive hyperæmia substances of a histamine-like nature are produced and it has been suggested that such a substance may be released during muscular activity. The elegant work of Dale and his colleagues on histamine reactions is so well known that no further mention is needed here. It has recently been shown that the anti-histamine drugs have a constrictive action on the capillaries.

It is usually assumed from the work of Landis that capillary pressures vary from an average of about 35 mm. Hg at the arterial end to about 12 mm. Hg. at the venous end of the vessel. This assumption is very useful for theoretical consideration of capillary function, but we must remember that the living body is a dynamic organism and statements of average figures such as these have little meaning except in certain carefully defined conditions. Clearly the capillary pressure cannot be less than that in the veins draining a given area. Recent measurements of the venous pressure in the dorsum of the human foot show that in the erect posture and standing perfectly still the pressure is approximately equal to that exerted by a column of blood from the level of the third thoracic vertebra to the point of measurement. The postural reactions necessary for normal standing are sufficient to reduce this pressure to an average of about 70 mm. Hg. When walking on a treadmill the pressure falls within three or four steps to a level of about 22 mm. Hg. In an atmosphere where the temperature is such as to produce sweating these venous pressures are higher. In those with varicose veins and whose valves are incompetent the venous pressure in the foot never falls below about 50 mm. Hg in the erect posture even when walking. We can conclude therefore that capillary pressures in the human being can vary from those measured at the level of the heart, i.e. 35 → 12 mm. Hg up to at least 70 mm. Hg in the foot in the erect posture. A full realisation of these wide variations in pressure is vital when considering the factors concerned in capillary permeability.

The classical theory advanced by Starling in 1895 to explain the balance maintained between blood and tissue fluid is intellectually very satisfying. His accurate assessment of the importance of the osmotic pressure of the plasma proteins has stimulated much work along these lines and has led to an understanding of some types of oedema. This theory in its simplest form assumes a hydrostatic pressure of 35 mm. Hg at the arterial end of a capillary, which being some few millimetres higher than the colloid osmotic pressure of the plasma proteins, allows filtration of water and crystalloids to occur, whereas at the venous end of the vessel the hydrostatic pressure has fallen to about 12 mm. Hg while the colloid osmotic pressure has risen by an indeterminate margin consequent on filtration, therefore fluid is reabsorbed from the tissues. Thus under so called normal conditions the balance is in favour of absorption. This simple physical explanation would be ideal if only the facts agreed.

The consideration of venous pressures in the foot of the erect human being, mentioned above, shows that even at the venous end of the capillaries under these conditions the hydrostatic pressure is higher than the colloid osmotic pressure and, therefore, filtration should occur all along the vessel. It is probable that this does occur in soldiers when standing in the unphysiological position of 'attention' on parade, particularly in warm weather. There is evidence of hæmoconcentration and fall of blood pressure in conditions such as this ; the former no doubt being produced by filtration in the lower extremities although there is as yet no accurate information of the amount of œdema produced. In quiet standing such as 'standing at ease' in military terminology there is no evidence of either hæmoconcentration or œdema production. This is fortunate for us in a country so addicted to queueing ! Lest it may be thought that tissue fluid pressures rise in

these circumstances sufficiently to produce a condition of turgor so great as to inhibit further œdema, it may be recalled how readily cardiac œdema returns when such a patient becomes ambulatory before compensation is effected. The only occasions in which comparable œdema occurs in the feet of otherwise healthy persons is in conditions of prolonged exposure to damp and cold such as gave rise to trench feet in World War I. It would appear therefore that the Starling hypothesis cannot explain why marked œdema does not develop on standing. Direct measurements of ' tissue pressure ' in subcutaneous tissue made by inserting a needle, give readings of a few centimetres of water and even after prolonged venous congestion or prolonged standing this pressure seldom rises above 10–12 cm. water. McMaster's skilful technique for measuring the force needed to drive fluid into the subcutaneous connective tissue spaces in man (called by him interstitial resistance) gives figures higher than this. In the skin of the dorsal surface of the ankle (which he admits is more tense than in most other regions) the interstitial pressure rose from an average of 3·2 cm. to 26 cm. of H_2O after about 30 minutes standing. The tissue pressure within leg muscles during prolonged venous congestion varies from 20 to 50 cm. H_2O depending on the toughness of the investing fascia. Pressures of this magnitude are enough to stop filtration into muscle during standing, but in the subcutaneous tissue the pressure is too low to combat the filtration predicted from the high venous pressures mentioned. Measurement of filtration at varying congestive pressures by Krogh and Landis using their ' pressure plethysmograph ' showed that venous pressure has to be raised to 17 cm. H_2O before filtration occurs in the human arm. The small amount of filtration produced was determined in terms of c.c. per 100 c.c. arm and they made the assumption in thus expressing their findings that the filtrate was evenly distributed throughout the tissues within the plethysmograph and obviously from consideration of tissue pressures mentioned above, the muscles (and bones) could have played no part in filtration. A further difficulty of a minor nature presents itself over the proteins which leave the blood stream. We have no accurate figures either of the total amounts passing out in unit time, or of the varying amounts from different tissues, or from the same tissue during differing phases of activity. It is usually assumed that this protein returns to the blood stream by means of the lymph, but there is ample evidence that some of it is utilised by the tissues. On examining the lymph we find, as Starling himself showed, that the amount of protein in the lymph varies from organ to organ. If we consider the condition of affairs in the intestine, where the capillary pressure owing to the portal system is somewhat higher than in most other places measured, we find also a high protein content in the lymph. Let us suppose that the pressure at the venous end of intestinal capillaries is 20 mm. Hg (as it may well be in the sitting or erect posture) and that the lymph or rather tissue fluid contains 4 per cent. protein and further that the flow of blood in relation to tissue fluid formation is sufficiently large to have little effect on the plasma protein content of say 6 per cent. In such a case the osmotic conditions are one third that of normal, i.e. only 10 mm. Hg osmotic pressure gradient. This would still leave a filtering force of 10 mm. Hg at the venous end of the capillary and we should expect to find a permanent œdematous condition of the alimentary canal which as we know is not normally found. This simple example is fraught with difficulties. The common assumption that lymph protein has been derived entirely from plasma has no factual basis. It is quite feasible that the lymph capillaries themselves manufacture proteins, for after all their progenitors in the embryo manufactured the first plasma. (Is it possible that the synthesis of proteins by the lymphatic endothelium is one of the means by which tissue fluid is ' pulled ' into the lymphatic capillaries?) The peristaltic action of the villi affects the flow of blood and must also affect the pressures within the vessels. During contraction the blood is squeezed out of the minute vessels and the wall becomes blanched, this is followed by an inrush of arterial blood into a system where capillary and venous pressures are at a zero level. These alternating conditions are maximal when the

intestines are fulfilling their digestive functions and no doubt in themselves aid absorption from the canal.

In muscle, when as previously mentioned capillaries may be almost a millimetre in length and following a more or less straight course, the concept that water is filtered off at the arterial end implies that a current of water must be streaming through the tissue spaces to allow of absorption taking place at the venous end of the vessel. Further, as Krogh pointed out, the tissues around the arterioles would be permanently in a semi-œdematous state. Considerations such as these illustrate how difficult it is to explain exchanges of a highly diffusible substance like water on the simple but brilliant concept of Starling.

The injection of dyes of varying molecular weight has provided useful information about capillary permeability. If a solution of a colloidal dye such as chicago blue 6B or trypan red be injected intravenously and the animal be left for about 20–30 minutes and then killed and exsanguinated a transverse barring, of red or blue as the case may be, is evident in skeletal muscle. On careful investigation the patterning is seen to occur around the transverse venules mentioned earlier. When this state of affairs is investigated microscopically the progress of the dye as it reaches the vessels gives a clue to the micro-anatomical structure of the vessels. The dye is seen to pass from artery to capillary and thence to venule, so it is possible to identify accurately which part of the system one is examining. After a lapse of time the dye is seen to escape from the distal end of the vessel. Later a faint mist of dye begins to appear all along the capillary. When the experiment is repeated with non-colloidal diffusible dyes a different picture is seen. These dyes leak out of the vessels at a much greater speed—judged in seconds rather than minutes ; and are first seen to pass out of the arterial end of the capillary and as the dye passes along the vessel it continues to diffuse, but for a brief moment a stage is reached in which even here a denser coloration is observed around the venular end. When this same experiment is performed on an animal whose blood pressure has been considerably reduced by means of successive bleedings the transverse bands of blue dye (using brom-phenol blue) are again found around the venules. In these circumstances there will be a great compensatory vasoconstriction in an endeavour to maintain blood pressure and one may suppose that in the contracted vessels two conditions exist which may favour this type of distribution ; (i) the capillary pressure is much below normal, (ii) the walls of the vessels will be somewhat thicker. A consideration of these findings led to the theory of a gradient of permeability along the vessels. It is interesting to note in passing that no such gradient could be elicited in the wing muscles of birds; subsequent examination of the microscopic anatomy of the circulation in these muscles showed a different picture from that of mammals. In the bird the ultimate arterioles are accompanied by the collecting venules, usually two venules to one arteriole and the distance between arterioles is considerably shorter than in mammals. The circulatory conditions here are such that one set of capillaries is carrying blood from right to left while another set is carrying blood in neighbouring vessels in the reverse direction, therefore although a gradient may exist in these capillaries it cannot be demonstrated because one set of capillaries will obliterate the effects produced by the other. Landis who used micro-injection techniques claims that dyes escape equally all along the vessel walls. Mention was made earlier of erroneous conclusions reached as a result of prolonged microscopic observations of exposed tissues. The findings that gave rise to the theory of a gradient of permeability can be obtained in an unæsthetised animal that has only one abnormality, viz. that a dye—which may be completely innocuous —is circulating in its blood stream. In Landis's experiments the mesenteries of the animals he examined for periods of an hour or more were bathed in Ringer's fluid and subjected to sufficient illumination to enable observations of capillaries to be made, further, micro-needles pierced the vessels, one can scarcely call these conditions normal. One criticism of the gradient holds that the collection of dye around the venous end of the capillary is

due to the absorption of water here, thus dye and water escaping at the arterial end are carried along outside the vessel and the dye concentrated at the venous end by the absorption of water. This criticism in itself involves a gradient in the reverse direction, i.e. substances escaping at one site cannot return at another. The fact that these colloidal dyes have a high osmotic pressure relative to plasma proteins was not considered and obviously as the dye gets concentrated in the tissue spaces it will oppose the plasma proteins by holding on to water. It would appear at first sight that in skeletal muscle the length of the capillary necessitates such a gradient for the transfer of nutritive substances of a high molecular weight. For assuming that the wall of the vessel was equally permeable throughout its length, then, as diffusion occurs, the vascular concentration would get progressively less as the blood flowed along the vessel; however it is still possible for a 'build up' to occur in the tissues which would oppose diffusion locally and enable it to take place further along the vessel and so on. Similarly diffusion could occur in tissue spaces, but here the distance is such—up to 1·5 mm.—that it would be of little physiological value.

Another view holds that water and salt escape through the cement substance which binds the endothelial cells. This is based on evidence obtained by locally outlining the endothelial cells with silver nitrate by means of micro-spray pushed through the mesothelium. On subsequent examination the blackened silver salts get washed away by fluid oozing out between the cells. It was also noted that intravascular carbon suspensions stick to the cement substance either through its inherent stickiness or else by being filtered off as water escapes. Consideration of the relative areas occupied by the cells themselves and by the cement substance would show how minute an area of the vessel is available for tissue transference if this takes place via the cement. Other workers have called attention to leakages that can occur in capillaries when micro-injections at a high pressure are made. It is probable that these leaky areas can be identified with the cellular interstices mentioned above and that an insult of this nature would disrupt this so far hypothetical substance.

The older histologists produced drawings of capillaries illustrating supposed pores of measurable size. These have since been shown to be artefacts. The idea of pores in the vessels, however, still fascinates the minds of workers in the capillary field but whereas these used to be measured in microns they are now measured in molecular dimensions of Ångström units. The awkward fact that proteins escape from the blood stream has only too often been overlooked. That the protein in lymph and perhaps therefore in tissue fluid has an albumin-globulin ratio not much higher than in blood presents a difficulty when explaining spread of these substances through hypothetical pores. The molecular weights of the smaller of the globulins is some three times as big as that of the albumins and with fibrinogen the ratio of molecular weight is 7/1. Some of the antibodies are of even greater size and they must diffuse out of the blood if they are to fulfil their ubiquitous function. The ingenious mind turns to modern physico-chemical discoveries to explain away this difficulty. The work of Cohn and his assistants has given us knowledge of the size and shape of these molecules and concludes that though they may vary in length they are more or less of the same equatorial diameter; therefore if a pore is big enough to let an albumin molecule slip through then why not fibrinogen. Such is the reasoning. When plasma is filtered in the laboratory it is common experience that the rate of filtration falls progressively doubtless because the proteins clog the filter although the meshwork or pores of our filters are immeasurably bigger than the hypothetical capillary pores. This may be stretching a physical analogy too far into a biological field but to my mind if we are to accept this theory for the capillaries then something similar must be postulated to explain the exchanges that occur in most cells of the body. For example there are several hormones of a protein nature which circulate in the blood stream and must diffuse out into the tissue spaces; are we to assume that only the cells of the target organ have pores of the right dimensions to admit these complex

proteins? (I know that this is somewhat childish reasoning but then children are usually logical and require evidence before accepting explanations of natural phenomena.) We have allowed our minds to be too concerned with structural details which fit this or that theory when thinking of capillaries and their functions. We have thought of these tubes with their exquisitely thin walls (less than 1 micron) as conveniently placed channels through which filtration can occur and have tended to forget that they are made up of living cells with a membrane (or rather two membranes) through which substances must pass; shall we not get further in our understanding if we regard these cells and their permeability as being subject to the same physico-chemical laws as that of other cells in the body?—remembering that internally they are exposed to a small hydrostatic pressure and that their inner and outer membranes are very much closer together than most other cells, and further, in common with other living cells possess the ability to alter their permeability to meet the requirements of the moment.

With this in view let us examine some of the results of recent investigations. For years it has been claimed that calcium in some way or other was concerned with capillary permeability—deficiency of calcium producing an increase in permeability. Chambers and Zweifach have reported on the effects of calcium on the cement substance, perfusion of solutions deficient in calcium washing away the cement substance, whereas excess calcium leads to an increase in cement and indeed a general stickiness of the entire endothelial cell. In the realms of cell physiology calcium is stated to be necessary for normal permeability to be maintained, the calcium in some way or other entering in a complex manner into the hydrated protein mono-layer of the cell membrane. Looked at in this way there is no need to invoke pores in either the endothelial cell or in the cement substance between them. No doubt we shall be hearing this morning from Professor Hevesy about his work on radio-isotopes. His experiments on the passage of heavy water from blood to the tissues rules out any question of filtration, for using this technique he was able to show that heavy water escapes from the blood so fast that in a period of 30 seconds it is distributed over a space comparable to that occupied by the extracellular water. Further his experiments on radioactive sodium and potassium would indicate that diffusion alone cannot explain this phenomena. From the ordinary laws of diffusion we should expect sodium to travel through the endothelium at a much faster rate than potassium, whereas he found that potassium moved through the wall at a rate five times faster than sodium. I shall be interested to hear what he has to say on this matter, but it seems to me that we have a problem here comparable to that seen in modern concepts of cell biochemistry. It has been shown in this field by means of isotopes that elements can leap in and out of protoplasmic structures with the greatest ease; becoming for a time, part of the living protoplasm. May it not be therefore that substances traverse the cytoplasm of the endothelium by dissolving in this fluid medium, and their passage out on the other side being determined by diffusion pressure gradients existing at the moment. Potassium ions being the 'normal' ion for the cell cytoplasm should theoretically be more acceptable to the cell and travel from unit to unit of cytoplasmic structure more readily than, say, sodium which is a 'stranger within the gates.' Some such conception is necessary to explain Hevesy's findings. To return to water. There must be diffusion into endothelial cells—and for structural reasons into the cells comprising arterioles and venules also—from both sides of the membrane all along these small vessels. Under normal conditions of plasma protein concentration the osmotic forces will favour the retention of water within the vessels but the molecules of water could be readily exchanged. Thus the water leaving by the veins may be identical in volume but not in molecules with the water arriving by the arteries.

All available figures of thoracic duct flow show how small this is at rest, viz. in the order of 0·5 cc. lymph per kilogram body weight per minute; we must therefore assume that under resting conditions on balance the amount of water escaping from the blood stream is exceedingly

small. The effects of tissue activity on capillary dilatation were briefly mentioned earlier in this talk, some recent work by Maurer is worth mention for it throws light on factors which influence permeability. He attempted to ascertain the differential effects of anoxia and excess CO_2 on lymph production. In anæsthetised and curarised animals maintained by artificial respiration he showed that when oxygen saturation of the blood was reduced to 75 per cent., lymph flow increased considerably, reaching maximal values when 52 per cent. O_2 saturation was reached. The protein percentage content of the lymph fell but overall, owing to increased lymph formation, a greater mass of protein left the blood in unit time ; the amount of protein passed into the lymph was sufficient to depress the serum protein content. Similar results were obtained when comparable tissue anoxia was produced by breathing CO mixtures. It seems to me that these experiments give a pointer to a possible explanation of variations in capillary permeability. In another field Maizels has shown that energy is needed if the erythrocyte cation content is to be maintained at its normal physiological level, storing of erythrocytes in the absence of sugar leading to a loss of potassium and a gain of intracellular sodium. May it not be that the endothelial cell, normally placed to have the first pick at any available oxygen, is unable to carry out and control its function of tissue exchange in circumstances when energy production is depressed. Certain it is that any insult, be it chemical or physical, results in the loss of semi-permeability that is the striking attribute of these thin ' diaphanous tubes.'

At the beginning of my address I said I would give an outline of my own views in this field. This I have tried to do but conditions of space and time have necessarily curtailed this endeavour. I have, however, in the time available said sufficient to bear out another opening remark, viz. how little is known or, perhaps, what is more correct how little your President understands about the physiology of the capillaries. My friend Kenneth Franklin has written for us an historical review of physiology in the past hundred years and hence I have not intended this address to be historical. I would however like to finish with a historical note—with which I am in sympathy—which was read by Beale before the Royal Microscopical Society on December 6, 1871 :

' In these days investigations must be conducted with such haste, and new facts discovered so quickly, that there is little chance of getting many persons to spend sufficient time in mere practice to enable them to gain the requisite skill for the very much more minute investigation of the structure of the most delicate textures which is now so much required, and which must be carried out before we can hope to arrive at positive conclusions on fundamental anatomical questions of the greatest importance.'

PSYCHOLOGY AND THE LAITY

Address by

PROF. C. A. MACE

PRESIDENT OF SECTION J

IT is not given to every age to witness the birth of a new profession, but in the century between the two Exhibitions we see the emergence in this country not only of the science but also of the profession of Psychology. The Great Leviathan has grown a new organ. To the two long established ministries, the Ministry of the Soul and the Ministry of the Body, a third has now been added—the Ministry of the Mind.

The professional psychologist did not create himself. He came into being to meet public demands, and to assume responsibilities which do not fall entirely within the province of either religion or medicine. These responsibilities, too, are for the welfare of individuals from the cradle to the grave. The professional psychologist is first of all invited by young and anxious parents to pontificate upon the problems of potting, weaning and temper tantrums. He is consulted by Educational Authorities on the allocation of children to their appropriate schools. Then he is asked to give vocational guidance. In industry he is called on for advice on the acquisition of skills, on problems of labour wastage, of supervision and management, on incentives, and on all the other conditions of productivity and industrial morale. Last of all he ministers to the profitable employment and the enjoyment of fading energies and talents in old age. The professional psychologist looks at these questions from the varied points of view of many occupations. He looks at them again from the point of view of the Forces, and from the points of view of the various branches of the Civil Service.

To make quite sure that his terms of reference are complete he is asked to keep an eye on every form of human waywardness. When things go wrong he is asked to put things right—to remove the causes of delinquency, to reduce social tensions and to abolish war. On all these matters, in the absence of better advice, what he says goes—or very nearly goes ; for added to all his other burdens he has to meet and mollify his critics.

Yes, it must be admitted that the professional psychologist has his critics. It is indeed not surprising that the emergence of a new profession, impinging as it must upon the vested interests of sagacity and experience, should engender some anxiety in the wise and the good. ' I am scared stiff of psychology ', a professor of philosophy has recently exclaimed. ' What is this new priesthood ? ' asks a constitutional historian. These expressions of inquietude have been echoed in the higher councils of the State. When powers have been sought for the professional psychologist to pursue what seems to him his innocent vocation there has been thunder on the left and more Olympic rumbles on the right. ' Are your mental tests,' he is asked, ' really consistent with a democratic system of education ? Are your newfangled Country House techniques well founded enough to allow you to usurp the right of the wise and the good to nominate their heirs ? You psychologists with your innovations want to turn the world upside down.'

To this the psychologist might be tempted to reply that he is not as yet convinced that the world at present is the right way up ; but this short answer is not good enough. If he knows only the elements of his job the psychologist knows that doubts and anxieties must be fairly met. He can fairly meet them only by starting from the premises of his critics.

To the scared philosopher he replies :

'Of course you are scared of psychology. I am not sure that I am not scared of it myself. The physicist is scared of the atomic energy he has released. I have greater reason to be scared of the powers that reside in human nature. To date, human nature has done much more damage than the atomic bomb, and the powers for destruction in human nature are by no means exhausted. We must chose between allowing human nature to continue its destruction and making some attempt to get it under control.' To the constitutional historian he replies : 'You know full well, better perhaps than I do, that the preservation of a civilised society depends on checks and balances, and upon promoting co-operation between those who agree on ends but differ as to means. As a social psychologist—and to-day all psychologists must be social psychologists —I, too, have to try to establish a system of checks and balances—including checks and balances for my own profession. After all, " that power corrupts " is first and foremost a pyschological proposition.'

The social psychologist cannot think of a science merely as disembodied doctrine. Sciences exist in the minds of men. Men have personalities. They live and work as members of groups, and groups too have personalities or 'patterns of culture.' The collective personality of a group of professional scientists, the relations within such groups and the relations of these groups to other groups in society as a whole, determine the social function of each science, and they determine the extent of its responsibilities and powers. That science, as such, is a bulwark of human liberty is not an *a priori* truth. It is not difficult to imagine a social order in which a body of scientists would take on the characteristics of a sacerdotal or political tyranny, or in which some arch-physicist or an arch-psychologist might wield all the powers of a dictator. That this could not happen here—here in Britain—is not because it is unthinkable, but because political and scientific institutions have developed, in the course of our history, as mutually reinforcing systems. There is, in short, a social psychology—or if the term be preferred, a sociology—of science. Within this science of the sciences some of the most intriguing problems are those that concern the relations of a professional body to its own distinctive laity. It may not be true that a country always gets the government it deserves, but it may be said with confidence that the health and efficiency of any professional body depend in some measure on the quality of the laity it serves. The health and efficiency of the *profession* of psychology depends first of all upon the integrity of the *science* of psychology. It depends on many external checks and balances. It depends upon the existence of a critical and educated laity.

The profession of psychology stands, I believe, here and now at a parting of the ways. One of these ways is that which has been taken by the ministry of our frail bodies. The other way is that which has been taken by the ministry of our immortal souls.

The medical profession has decided on the whole that its public shall not be informed. Its laity is of course encouraged to understand the elements of hygiene. It is not encouraged to occupy itself with medical science proper, or to indulge in anatomical introspection. In its impenetrable wisdom the medical profession has decided that it is best if the patient cannot read its prescriptions. The patient can exercise his patience in the confident assurance that his doctor knows, and is doing, what is best. Far be it from me to question these decisions.

In the other great ministry the case is otherwise. The Gospel shall be propagated. Every layman shall be encouraged to understand, each to the limit of his powers of comprehension. The richness of English prose owes a debt greater than can be paid to the ministry of religion for telling all that can be told in words that all may understand. True, there are the mysteries of religion, but these are mysteries to bishop, priest, deacon and layman alike.

Between these two ways the professional psychologist has now to make his choice. For my part I have no doubt what this choice should be. The professional psychologist writes no prescriptions. He has no secret remedies. The good that he can do he can do only with the full and free co-operation of an educated and critical laity. A policy of collaboration with the

laity is, I believe, right, not only for the profession of psychology but for the science, too. British psychology has, in this respect, a great tradition to preserve. I will try to explain what I mean by a brief historical excursion.

The new profession is based upon a new science. As the profession is *fundamentally* new so was the science *fundamentally* new. Psychology differs from most other sciences in ways in which these other sciences do not differ from one another. It differs not only in its form and features but also in its mode of genesis. I do not know how long may be the gestation period of a science, but the prenatal procrastinations, in the womb of philosophy, of the embryonic science of human nature were sufficiently protracted to engender some discomfort in the mother and some anxiety in the midwives. The child was, however, eventually delivered (circa 1851) and duly registered in the stud book: *British Psychology*, out of *British Philosophy* by *British Natural History*.

To change the metaphor, British Psychology in the mid twentieth century lies just below the confluence of two great streams of reflection and research. The first of these, which has the longer reaches, has its source in the intellectual highlands of philosophical speculation. We can trace it back in this country to the work of Thomas Hobbes. This stream flows through the British Empiricists, Locke, Berkeley and Hume. It begins to assume a scientific form in the writings of David Hartley. Thence it passes through the two Mills and Alexander Bain, increasing in breadth and depth and acquiring a new vigour in the works of our near-contemporaries, James Ward and George Frederick Stout.

The second great stream derives from the British naturalists—men who studied the bee, the field-mouse and the worm, and through their studies of the bee, the field-mouse and the worm were led to study man.

The prenatal history of the *science* of psychology could almost be written from the biographies of three generations of the Darwin family. These Darwins were first and foremost humanists and amateur naturalists—the laymen of the science of biology. We can trace their intellectual pedigrees to men like Isaac Walton, the ironmonger of Fleet Street, who enjoyed the friendship of Donne, and to men like Gilbert White, of Selborne, who ministered so happily both to the human souls and to the field-mice in his parish. They differed however in being experimental naturalists. They seem to have been endowed in double or triple measure with the itch to observe, to understand and to explain, to experiment and invent, to think thoughts which had not been thought before and to do things that have never been done before. They could not observe a fact without asking: Why? They could not see anything done in a certain way without raising the query: Why not do it in a different way? In some of them the bent for practical contrivance is the more pronounced, in others the urge to explain. In that eccentric and monumental genius, Dr. Erasmus Darwin, the two propensities were equally strong. In his grandson, Charles, we see this genius disciplined, canalised and directed to a single main objective. In the other distinguished grandson, Sir Francis Galton, the versatility and inventiveness of Erasmus Darwin reappears, but this time to the greater advantage of psychological studies.

It is a fact of some importance for what I have called the Sociology of Science that the first of these precursors of psychology lived before the great divisions yawned between the arts, the sciences and the technologies. Erasmus Darwin was a poet (of sorts), a naturalist and a technologist. His intimate circle included men to whom to make and do and invent was the breath of life—Wedgwood, the potter, Boulton, the engineer, Priestley, the chemist and Watt of steam engine fame. But the number of things that Erasmus Darwin invented equalled those of all the others together. He was a versatile excogitator of hypotheses and an irrepressible designer of contrivances. His eighteenth-century house was as full of nineteenth-century gadgets as his head was full of twentieth-century ideas. A weather-vane on the roof controlled a dial in his study. He installed a speaking tube to his kitchen, and his garden contained one of the first artesian wells. He invented ingenious lamps and candlesticks, a loom for

knitting stockings, a duplicating apparatus, a surveying machine, a talking machine and a mechanical flying bird. On the grander scale, he had schemes for locks and canals and a plan for improving the weather. He is known best to science for his anticipation of the Lamarckian theory of evolution but perhaps deserves most to be remembered for the theory—expressed both in verse and in prose—that plants, too, have minds. His *Plan for the Conduct of Female Education in Boarding Schools* is characteristic in its humanity and in its psychological good sense. He anticipated some of the ideas of Samuel Butler and Bernard Shaw, and it has been suggested that he had anticipated Freud. Had he been alive to-day he would certainly have been among the devotees of Cybernetics. If David Hartley is accounted the founder of physiological psychology, Erasmus Darwin must be accounted the first among the moderns to conceive the science of mind as part of the wider science of life. His *Zoönomia* must be included among the source books of the historian of pyschology. His greatest legacy to British science was, of course, his grandson, Charles.

The contributions of Charles Darwin to biology in his major works have tended to obscure his contributions to psychology in the *Expression of the Emotions* and the *Biographical Sketch of an Infant*. His massive scientific publications might fairly seem to jeopardise what I may be pardoned for describing as his amateur status, but I like to think of Charles Darwin, not only as writing the *Origin of Species* but as engaged at the same time in rustic pursuits and in family life at Downe. Whilst Huxley was 'having words' with the bishops on Darwinian theory, Darwin himself was helping the vicar of his parish to organise a Friendly Society and acting as the treasurer of the Coal Club. He was arousing the interests of gardeners in the habits of their worms. He was breeding pigeons as a member of the Philopedestera and Columbarian Clubs, and writing exuberant letters about the incapacity of the human intellect to encompass all the virtues of the Almond Tumbler. All this is what preserved his amateur status—this and the simplicity of his experiments. His laboratory was his study and the potting shed. His instruments were few and of a homely, makeshift kind : saucers and biscuit tins, presumably borrowed from the kitchen, and bits of coloured thread for distinguishing plants, presumably borrowed from Mrs. Darwin's workbox. These borrowings were not all on one side. The children raided the study for string, pins and scissors, for footrule, sticky plaster or hammer. They joined in his experiments. When Darwin found that the cotyledons of *Biophytum* were highly sensitive to vibration of the table, he got the idea that they might be sensitive to sound. So Leonard was called in to play his bassoon. The bassoon was also used to test the hearing of worms. The bassoon technique is in the best tradition of British amateur science. A century before Gilbert White had tested the hearing of bees by shouting at them through a speaking trumpet. And this tradition has been preserved almost to the present day. It is well within the span of short memories to recall how in our premier psychological laboratory ingenious experiments were conducted with contraptions devised of scraps of cardboard, sealing wax and string, supplemented from the resources of one of the less expensive of Meccano sets. The stories of Erasmus and Charles Darwin belong, however, to what I have called the prenatal history of psychology. In the work of the other distinguished grandson of Erasmus, British Psychology was well and truly born. Francis Galton was a naturalist too, the greatest perhaps of what we might call the *human* naturalists (because we need a word for those who study men as the older naturalists had studied the field-mouse and the bee).

In the encyclopaedias he is described as an anthropologist, and like most of the great precursors of psychology he was a travelled man. Some sort of 'naturalist's voyage' was in the education of the young scientist of the nineteenth century what the 'grand tour' was in the education of the young nobleman in the century before. In 1851 Darwin had returned from his voyage on the *Beagle* and was already well settled at Downe. Huxley was just back and looking for London rooms, complaining about the effect upon rents of 'this confounded great Exhibition.' Galton was still among the Ovampo in South West

Africa. He returned in the following year, but it was not until 1865 that he published his first studies of *Hereditary Talent and Character*. But from then on his versatile mind and his consuming curiosity were continuously occupied in anthropometrical, statistical and psychological studies. But somehow or other he managed to combine with these the pursuit of many and diverse inquiries. He was like his grandfather in the versatility of his curiosity, in the ingenuity of his invention, and in the vastness of his projects. Erasmus Darwin had had a plan for improving the weather: Galton had a plan, called Eugenics, for improving human nature. Incidentally, however, he established the existence of anticyclones. It may accordingly be said that if he did nothing to improve our British climate he at least helped us to understand it—and so, in some measure, to forgive it.

As a meteorologist he naturally thought in terms of data collected over a wide area. He thought of psychological data in the same way. He persuaded schools to keep some elementary anthropometrical records. He even started to make a Beauty Map of the British Isles and proved that at that time (and, of course, so far as as we know, only at that time) the girls in London were prettier than the girls in Aberdeen. He laid the foundations for scientific vocational guidance. The leading idea of the laboratory he conceived was that its measurements 'should effectually "sample" a man with reasonable completeness, and ascertain the degree with which the measurements of sample faculties in youth would justify a prophecy of future success in life.' The laboratory —the first in this country—was established, characteristically, at an International Exhibition. It had what I suspect is the unique distinction of being self-supporting. He writes with quite excusable complacency that 'the ease of working the instruments was so great that an applicant could be measured, a card containing the results furnished him, and a duplicate made and kept for statistical purposes at the total cost of a threepenny fee which just defrayed the working expenses.' It makes one wonder whether we psychologists of 1951 have not missed an opportunity. Be that as it may, Galton's laboratory in the International Health Exhibition of 1884 may fairly rank with that founded by Wundt at Leipzig five years earlier as one of the landmarks in the history of Psychology. It was, at any rate, the first field station of the experimental human naturalists.

It would not be easy to point to any field of interest in psychology to-day which owes no debt to some or other idea which passed through the mind of this remarkable man. To him we owe the basic concepts of mental testing and the basic statistical procedures by which the data of tests must be dealt with. He was, I believe, the first to use time-sampling techniques for the study of behaviour. He devised a method for the measurement of boredom by counting the number of fidgets per unit of time. His unwitting subjects were the learned members of the Royal Geographical Society (for even there, he dryly remarks, dull memoirs are occasionally read). He anticipated our opinion surveys and our studies of group decisions. He obtained evidence that on certain matters of fact the *vox populi* could be accurate within a limit of about 2 per cent. error. To his ingenuity we are indebted for many of the historic pieces in our psychological laboratories. His inventions are outstanding by their simplicity and fitness to their purpose. Whilst others designed elaborate contraptions for the measurement of reaction times to insignificant decimal places his was a pretty little contrivance—it may still be seen at Bedford College—which was content only to answer the question that could be usefully posed. When his instruments are forgotten he will be remembered for the wealth of his curious observations. The *Inquiries into Human Faculty* and his *Memories* can still be recommended to the student as a corrective to the systematic text-books. Some might dub their contents 'anecdotal' but the anecdotes are quite exceptionally good.

It is not my intention here and now to review the history of British psychology from Galton to the present day. This has been done, and so superbly done, by my predecessor in this office. In Dr. Flugel's *Hundred Years of Psychology* you can read this story at your leisure, and at your leisure try to assess the relative importance

of our own psychologists' contributions and of the contributions of those from overseas to our present situation.

Nor is it my intention to review this present situation. In the Section's programme for the coming week those most actively concerned in what is going on in the major fields of research will tell you themselves some of the things that they are doing. I propose to spend the short time that remains at my disposal in speaking of one elusive trait which the spirit of British psychology of to-day shares with that of the age in which it was born. In the first half of the present century psychology, in common with other sciences, has derived a tremendous impetus from the impact of two titanic wars. In and between these wars the most pervasively influential personality, of psychologists working continuously in this country, was that of the late C. S. Myers. The elusive trait to which I have referred is I think one that is displayed in the spirit of Myers.

In the life and work of Charles Samuel Myers we see another efflorescence of the British tradition in psychology. To borrow the words with which Professor Pear ended his affectionate memoir in the British Journal of Educational Psychology, Myers will be remembered as a 'humane cultured scientist.' Like Galton he might be described as a 'human naturalist.' His scientific career, like that of most of his naturalist precursors, began with a voyage. He conducted a series of studies, of a rather Galtonian kind, into the faculties of natives of the Torres Straits. On his return he, like Erasmus Darwin, moved in circles which ignored, and by ignoring in some measure closed, the chasms that divide the arts, the sciences and the technologies. He played the violin, he wrote a text-book of Experimental Psychology, and he founded an Institute for the application of psychology to industry. What Britain can offer to the world in the century to come depends, I would suggest, upon preserving this tradition. And in preserving the tradition lies the answer to the scared philosopher, and to the anxious constitutional historian. It need not be the primary concern of the psychologist to appease his critics. This he will do best by getting on with his job. There is no reason, why he should not think of his job in quite a big way. One thing he might set himself to do is to reconcile the demands of what the philosopher calls, in his grandiloquent way, Truth, Beauty and Goodness. In work-a-day terms, it is the business of the scientist to find out the facts as they are. It is the business of the artist to study what Erasmus Darwin called 'gentlemanly and agreeable facts.' It is the business of the technologist to see to it that what is agreeable shall also be made true.

The professional psychologist can play his part in this task only with the full collaboration of an educated laity. I repeat, without apology: the psychologist writes no prescriptions, he has no secret remedies. Professor Valentine, it seems to me, has put his finger on exactly the right spot. 'You cannot lay down rules,' he said, 'for bringing up children.' He was advising the young professionals on their responsibilities to the anxious parents who so much need the guidance of psychologists on so many difficult problems—how and when to pot, how and when to wean, when to give rewards, and when to reprimand, when to ask for the child to be tested, when to ask for remedial teaching, when to do this and when to do that, *and when above all to leave the child alone*. It was, no doubt, with all these things in mind that Professor Valentine said: 'You cannot lay down rules.' His statement could be generalised: 'You cannot lay down rules for the supervision and management of children, you cannot lay down rules for the management of grown men.' Roethlisberger—of Hawthorne fame—has more recently said much the same thing: 'The science of human relations has no *principles*' was the way he put it. These statements may not be in strict literalness correct, but they are true enough. The truth certainly is that the rules and principles of psychology call for psychological understanding in their application: in the last resort it is the layman who has to make the application, for the chances are that only a layman will be on the spot when psychology needs to be applied. The psychologist exerts his influence indirectly; he exerts it through his laity. He operates, of course, from the vantage point of a psychological laboratory or an institute

for psychological research. But to-day a psychological laboratory or a research institute is not a temple in which consecrated experts practise esoteric scientific rites. It is first and foremost a base for field investigations. It is a house with many doorways, thronged with two-way traffic. Men come in with problems, and each will go away, not with a ready-made solution, but some hypothesis to test. It contains the usual paraphernalia of workshops, laboratories in the narrower sense of the term, and it contains the accessories of statistical computation ; but among the most important of its chambers will be its conference rooms. Among those who come and go will be the professional scientists, not only the psychologists themselves but also physicists and cyberneticians, representatives of the other biological scientists, sociologists, anthropologists and economists. So, too, will come and go doctors and clinicians, naturalists, teachers and technicians, parents and their children. With equal rights of entry will be all of those who enjoy any form of skill or any kind of first hand experience in the human arts or in the technologies. They are not more anxious to confer with the psychologist than the psychologist is to confer with them.

Behind all these comings and goings there must be a clear and precise conception of the rôles of each of the major participants—the pure scientist, the technologist or professional practioner, and the pure laymen. In recent years much has been said about the need for ' multidisciplined operational research '—by which we may understand merely the combined attempt on the part of many specialists to solve practical problems in a scientific way.

It is, I believe, now generally agreed that rarely if ever can a particular kind of scientist, physicist, chemist or psychologist, solve a practical problem on the basis of his own science alone. A practical problem draws upon advice from more than one kind of scientist. What remains to be still more widely recognised is that the pure scientist as such, or even a syndicate of pure scientists, may not be qualified for the special work of applying or adapting the results of research in the field to which these results are potentially applicable. This is the function of another kind of specialist, the professional or the technological, who combines scientific training with relevant practical experience.

Still further, at present, from general recognition is the fact that in the application of science the so-called layman has a rôle to play. In applied psychology in fact, he has at least two : He, like the trained technologist, is also often a repository of practical experience, and he is the patient to whom the science is applied. So long as the application of science is left to the pure scientist (who often with the best of reasons can disclaim responsibility for the application of his findings) so long shall we continue to hear complaints of the piling up of unutilised results of research. The effective rate at which science is applied is the rate of consumer assimilation, and the rate of consumer assimilation depends upon specific organisation in which the experienced technologist and the experienced layman play essential parts.

The piling up of unapplied products of research is nowhere more evident than in the field of the human and social sciences ; and this I suggest is in the main due to the lack of professionals and technologists who combine with the relevant basic scientific qualifications the qualifications of experience and training in certain basic skills of application. In medicine the general practitioner is the technologist in question : in education it is the teacher. In industry there are highly trained technologists for the application of every science except the science of man. This is perhaps the greatest industrial need of to-day—the need for a trained professional body, occupational psychologists, occupational sociologists, occupational anthropologists—call them what you will—to do for industry what is done by the teacher and the general practioner in their respective spheres.

It is for these reasons we may hope to see in the flow of traffic to and from our psychological laboratories and institutes for research a goodly company of those engaged in the professions and technologies of industry and commerce—teachers from technical colleges, shop stewards, personnel officers and all who are in any way concerned with training, supervision and the human relations of industrial life.

Each such institution for research will

have, of course, its own distinctive field of interest. If I here select for illustration one contemporary interest it is not only because I think it one of the most important but also because it serves as well as any other to illustrate my theme—the need for close association between professionals and laity in the arts, the sciences and the technologies.

Among the preoccupations of this country at this time is the need for advance in higher technological education. You have only to scratch the surface of this problem, and to scratch it where you will, to find issues big enough to interest and to embroil not only technologists and industrial psychologists but other scientists as well, and not only scientists but also professors in the humanities, and not only all these professionals but also the laity of the arts, the sciences and the technologies and those who enjoy experience relevant to these issues. Scratch the surface, for example, of a problem raised almost incidentally in last year's paper by Sir Ewart Smith to this Association on *The Critical Importance of Higher Technological Education in Relation to Productivity*. In his diagnosis of the present situation Sir Ewart refers to the common belief in this country that a career in applied science in industry is in some way inferior to that in other professions, that it is considered a higher calling to carry out pure scientific research or to study classics or history than to apply knowledge to productive industry. To understand and to remedy this fundamental weakness of our civilisation—and I believe it is a fundamental weakness—would be a worthy object of prolonged research. It would be a bad mistake to dismiss the fact as yet another instance of human snobbery. It is a mistake to suppose that the remedy lies in any form of propaganda. Both the diagnosis and the remedy lie, I believe, in fundamental research into the basic conditions of 'status' and 'prestige.' That industry and commerce are so often thought of as things which the educated man will naturally wish to avoid is not an isolated phenomenon. A similar problem has been encountered in the attitude of scientists to appointments in the Services, and in many other places. A year or two ago, Professor Knight reported some penetrating observations on the 'reluctance to teach,' and some of his observations have been abundantly confirmed by studies on a larger scale. All who, like Professor Knight, are favourably placed for knowing what goes on in the minds of University students can testify not only to the reluctance to teach but also to the reluctance to follow some careers which might seem to hold out promise of greater financial reward. They can testify to the fact that among the stronger enticements to a career is the enticement of belonging to a certain class or group, the members of which enjoy *a certain style of life*. As it was put by a correspondent to *The Times* writing on the unattractiveness to graduates of permanent commissions in one of the Services: 'The real reason is . . . that they do not fancy the life.' As it was put to me by the Principal of a Technical College, parents (who still apparently exercise some influence on their children's choice of occupation) are apt to ask 'And if my boy follows the course you advise, will he get a job in an office, and will he have a telephone on his desk?' An office and a telephone can be symbols of a style of life.

Professor Sargant Florence has more than once reminded us that we need 'incentives to mobility'—that the problem is not so much to get a man to work when he has taken a job, but to get him to take the job at all. It is surely clear that in all this we have encountered a large and complex problem. Sociologists and economists have drawn our attention to a prevailing drift—from the face-to-nature occupations (such as farming, fishing and mining) to the manual factory operations, and through these to the office jobs. The drift from industry and commerce to the professions and the drift within the professions are but the end of this story.

There are, of course, 'historical reasons' for the drift to the West—the drift to the Inns of Court, to Whitehall, to Harley Street and Bloomsbury. But 'historical' reasons are psychological reasons too. History counts because at least one half of history is a record of the persistence and transmission of beliefs and attitudes. As co-operation in production has suffered from a history of poor human relations between management and the factory

floor, so both industry and commerce have suffered from a history of poor public relations between business and the professions. A bad patch of history operates in the life of a group in the same sort of way as a 'traumatic experience' operates in the life of an individual. The tragedy is that none of the features of industry by virtue of which the young 'do not fancy the life' are in fact essential to the organisation of production. What youth believes about industry may not even be true, but the causes here are not the facts themselves but what the facts are thought to be. To arrest the drift to the West it well may be that we must arrest the other large-scale trend to which I have referred—the divergence in the ways of life and thought, since the eighteenth century, between the sciences, the arts and the technologies.

What, it may well be asked, can the psychologist do about these things? First, he can set about testing, by all the resources of scientific method, the hypothesis here suggested, together with the alternatives that may occur to others.

In the second place he might set about testing the suggestion in a more homely and more 'practical' way. This psychologists can do by fostering in their own way of life the traditional association of these sciences with the arts and with the world of practical affairs, and by maintaining good public relations with their own laity.

I have spoken of a psychological institute as a place of many doors. All of these must open on to two-way streets. The life of such an institute can, I think, make its own distinctive contribution to a liberal education, for a liberal education might be defined as one that secures the greatest areas of contact between the greatest number of different ways of life. Two corollaries might be developed.

First, psychologists have a special responsibility for the maintenance of the British tradition in the popularisation of science. No apology is needed for the use of this word. It is in this tradition not only to maintain the highest standard of scientific journalism, but also for those who themselves make discoveries themselves to present them to the laity. A popular science series published in the nineteenth century contains among its authors: Huxley, Spencer, Lubbock and other distinguished scientists. These names recur in the *Nineteenth Century* itself. It was a British scientist who said that he never felt quite satisfied with a theorem until he felt that he could explain it to the next man he met in the street. It is in virtue of this tradition that the *British Association* is a British association—an association of professionals and laymen.

The second corollary is like unto the first. Psychologists have a special responsibility for the preservation of a common medium of communication. The recent demands voiced by distinguished civil servants, professors of English, and by the 'Working Parties' appointed for the good of industry for the use of 'Plain Words' and of 'Direct English' are not demands solely of utility or of aesthetics. They are demands of sociological, of anthropological and of psychological significance. Technical terminology may be used as an instrument of power politics, as a code, and an instrument of exclusion by the members of a cult. Like all legitimate instruments it can be employed for nefarious ends. This is a free country and scientists must be allowed to talk to one another in whatever language they like. They must be allowed to use what words they like, so long as they tell us what they mean. They must, however, try to be bilingual. In the case of psychology I do not believe the demand to be exacting. I myself am much less learned in psychology than many of my colleagues, but in my studies so far as they have gone I have not met a psychological fact or a psychological theory that could not be expressed, at the cost of a little reflection, in language which an intelligent schoolboy—up to School Certificate standard—can readily understand. Psychologists, I again repeat, and again repeat without apology, write no prescriptions and they have no secret remedies. All that they know they must tell. It is not for them to lay down rules but to communicate understanding, first of all to parents, then to teachers, and then to those concerned in any way with the management of men. The task is one for humane, cultured scientists, and a humane cultured laity.

If in the years to come we can preserve the memory of the humane, cultured scientists—of humane, cultured scientists with something of a technological bent—men who feel and do as well as think, neither philosophers nor historians need be scared. How thin is the line between the humanist of science and the humanist of the arts we are reminded by Ruskin's efforts to encourage the young to study natural history. His fondest dream, it has been said, was that British youth should prefer looking at a bird to shooting it. I suspect that the human naturalist also has his dream. Perhaps he believes that if we studied our fellow men we might be less inclined to shoot them.

Not only how thin, but also how twisted is the line that divides the humanist of science from the humanist of letters we are reminded by a remark of Leslie Stephen concerning Swift. 'Swift,' he said, 'studied the manners and customs of his servants as Darwin studied worms. The difference was that Darwin had kindly feeling for his worms.' Nor should the fact be surprising. Scientists are men, and as Francis Galton writes at the end of his memoirs: 'Man is gifted with pity and other kindly feelings: he has also the power of preventing many kinds of suffering. I conceive it to fall well within his province to replace Natural Selection by other processes that are more merciful and not less effective.'

If these things be remembered and the British tradition in science be preserved, the wise and good need suffer no alarms. There may yet come a day when all, even philosophers and constitutional historians, will say: Let there be more psychologists—*one* psychologist in every factory, *one* psychologist in every school, but first of all *two* psychologists at least in every home.

MYCOLOGY OVER A CENTURY

Address by

PROF. W. BROWN, F.R.S.

PRESIDENT OF SECTION K

THE central theme for this meeting of the British Association is stocktaking. We are at the half-century (or approximately so) and it has been suggested that we should do well to examine what progress our science has made in the past 100 years. In 1931, on the occasion of the centenary celebrations of the Association, there was a similar retrospective bias and the President of Section K for that year, Professor T. G. Hill, undertook the considerable task of tracing the advancement of botany over a century. I am glad to say that my field will be distinctly narrower and that I shall confine myself in the main to the mycological branch of botany. At times, and especially in the earlier part of this address, I shall be forced to extend my scope to include certain related branches, e.g. bacteriology—in other words to broaden out mycology into microbiology, that composite subject of which one has heard a great deal lately.

A century of course has no particular magic attached to it—it is merely a nice round number. But it so happens that the 100 years which have just elapsed represent very accurately the lifetime of mycology (and bacteriology) as a science. The story of 100 years' advancement in mycology is therefore to all intents and purposes the sum total of its history. Previous to 1850 there had been, it is true, a substantial amount of taxonomic work about which I shall say something later, but little else of scientific value had been achieved by that time, nor in the circumstances then prevailing could it have been otherwise. My first object will be to establish this point, and this leads me to outline what was the scientific view regarding the nature of micro-organisms at the middle of last century.

The ordinary informed person of to-day knows that fungi and bacteria play an all-important rôle in nature by breaking down organic material—such as the dead parts of plants and animals—whereby the constituent elements may again be built up into the bodies of new organisms. He also knows very well that many of these bacteria and fungi can be dangerous parasites, causing diseases which are often of an epidemic character. A hundred years ago the association of bacteria and fungi with disease and with the decay of plant and animal materials (putrefaction) was also appreciated, but with a very important difference. For whereas we now recognise fungi and bacteria as the causal agents of these effects, the whole weight or at least almost the whole weight of scientific opinion at that time was against such a view. These organisms, often called animalcules, were considered merely as the consequences of a diseased or a putrefactive condition, the cause of which was something else. Their association with death or decay was invariable for they were believed to come into existence by a direct transformation of the decaying substance. If they were organisms, and it was questioned whether they were organisms in the sense in which the higher plants and animals are organisms, they came into being *de novo*. This of course is the doctrine of spontaneous generation, widely held by eminent scientists up to the middle of last century and by some even later. To the modern mind this seems a very strange doctrine but it is not so unnatural as one might think, and there is at least one important truth concealed in it, which I shall point out later. As it is my aim to sketch the growth of ideas on the nature and functions of micro-organisms, and as it was obviously fundamental that a decision should be reached

as to whether these organisms existed, as it were, in their own right or were merely by-products of other organisms, I propose to look into the theory of spontaneous generation for a little while.

The theory is as old as history. A well known example of it is the generation of bees from the carcass of a lion, or other large animal. 'Out of the strong came forth sweetness.' Whenever any small animal appeared in unusual numbers, the idea of spontaneous generation was brought to bear, even though it is fairly certain that the natural process of generation was well enough known. Nor was it merely the common masses who held such views. It is surprising to learn that as late as the middle of the seventeenth century the famous chemist van Helmont published in all seriousness a recipe for the spontaneous generation of mice.

By the nineteenth century the theory had been abandoned for the more macroscopic animals or plants and had come down to the region of fungi and bacteria. Thus Ehrenberg, writing in the 1830's says 'Fungi are not plants at all; they are *lusus naturae*.' In the simpler view micro-organisms were supposed to arise in decaying material from a tendency of the latter to aggregate, in much the same way as crystals separate out of a mother liquor. Another view, which was more difficult to combat, was that when a plant or animal died small bodies (microsomata, microzymes) continued to live on, and that these developed under suitable conditions into fungi or bacteria.

It is worthy of note that, in the controversy which centred round the spontaneous generation of small organisms, some of the chief supporters were leading chemists of their day. Berzelius was one of them; another was Liebig, who died in 1873 and who maintained it all his life. When Cagniard Latour and Schwann (the author of the Cell Theory) independently and almost simultaneously (1836–7) claimed that yeasts or bacteria brought about the process of fermentation in sugar solutions these chemists ridiculed the idea in no uncertain terms and in a manner that was far from polite. They asserted that fermentation was a purely chemical reaction, caused by an unstable ferment which communicated its instability to certain organic substances. This in fact is true in a kind of way, but the ferment is derived from the micro-organism. It was not till 60 years later that this ferment was prepared from yeast and its properties studied.

There was a reason for this violent opposition to a biological theory. A few years earlier (1828) Wöhler had succeeded in synthesising urea in the laboratory. This was an outstanding achievement in chemistry, because the belief had hitherto been that organic substances could not be built up without the action of a 'vital force,' this force being something which transcended ordinary chemical and physical laws. The synthesis of a typical organic substance like urea removed the need for postulating a vital force, and the tendency therefore was to discount the existence of any real barrier between the organized and unorganized states of matter. In the light of this trend in chemical thought, it did not seem unduly optimistic to suggest that organic material could regroup itself and become organized, i.e. become living substance.

The period of 30 years from 1850 to 1880 was one of intense activity in biological research, more especially in Germany and France, and in so far as one can date the foundation of any science one can say that the sciences of mycology and bacteriology, as we now understand them, had their beginnings at that time. The classical researches of Pasteur established the biological theory of fermentation, and in doing so killed the theory of spontaneous generation as applied to fungi and bacteria. His studies on lactic acid fermentation appeared in 1857, to be followed six years later by equally significant work on butyric acid fermentation. Incidentally this is the first account in literature of an anaerobic type of organism (Pasteur invented the term 'anaerobe.'). I have already noted that the same views had been put forward 30 years earlier but they did not gain acceptance, either because they were not established beyond criticism, or because the timing was not ripe for the new idea—probably the latter in great measure, for that sort of thing has happened more than once. Pasteur's experimental genius settled the matter beyond all doubt and brought the scientific

world round to his way of thinking. It is perfectly right therefore to regard him as the founder of microbiological science.

In due course Pasteur went on to researches on the curing of disease, researches which led to one of the greatest contributions of science to the benefit of mankind. Contemporary with him in this field were the Germans Robert Koch and Ferdinand Cohn whose special contributions were to found bacteriological technique and systematic bacteriology. In mycology the great names were the Tulasne brothers in France, and the Franco-German Anton de Bary.

The sciences of mycology and bacteriology have so much in common that any advance in the one which leads to a new point of view is almost certain to have repercussions upon the other. As we have seen, they had their beginnings at much the same time. Their further histories follow much the same course, with parallel advances and now and then with similar phases of confusion and controversy. From now onward I shall largely confine myself to the more strictly mycological side, and to illustrate more fully how the subject stood at the beginning of our period, I shall have to go back some 20 years to the time of Elias Fries, whom most mycologists look to as the founder of systematic mycology.

Fries' *Systema Mycologicum* was published over the years 1821–9. It represented an enormous step forward in the classification of the Fungi. Any detailed examination of the scheme would be out of place here, and in any case I do not feel qualified to present it, not being a systematist. Even so it is impossible to browse through that monumental piece of work without astonishment at the knowledge, energy and enthusiasm of its author. In some respects it is quite modern, as far at least as concerns the larger fungi, and so it is that mycologists have come to accept Fries' authority for the naming of fungi to the same degree as his fellow countryman Linnaeus is the recognised fountainhead for the naming of higher plants.

There are however some features which concern my present purpose inasmuch as Fries' views dominated the mycological mind for a quarter of a century afterwards. In passing I would just note, as something which will surprise the mycologist who only knows the modern outlook, that Fries' Gasteromycetes group includes the slime fungi, Thamnidium, Sphaeria and Eurotium, over and above the puff-balls and such like. It is not difficult to see what the point of resemblance is, and it should be remembered that though the eight-spored ascus was well known by that time, no clear distinction was drawn between Phycomycetes and Ascomycetes. Also the basidium was not discovered (by Leveillé and Berkeley independently) until a few years later. But it is in the group which Fries calls Coniomycetes that the chief interest lies. The word means ' dust-fungi ' from their dust-like spores. A large section of this group is called Hypodermii by Fries, because they occur under the skin of plants, from which they break out with dusty air-borne spore masses. These are in fact plant parasites and they include the families of the Rust and Smut fungi. Now while Fries was very much at home with the larger fungi, he took a poor view of the Hypodermii. This is what he says : ' The old botanists (e.g. Malpighi) considered the Coniomycetes as Exanthemata or paid little attention to them ; lately their study has begun to flourish, to the detriment, if I might say openly what I feel, of the knowledge of the nobler fungi. Among the Hypodermii which constitute the greater part of them, let it suffice merely to recognize the general types, unless one wishes to dedicate ones life to the different forms which are to be seen in various plants. All study of them seems to me to be physiological frustration. At least that is my private opinion.' He goes on to add that the more they are studied the more do their specific boundaries become uncertain, and they are quite unlike the higher fungi which can boast of a continuity in their history as species. Another of his verdicts is : ' These fungi depend on a diseased condition of the plant rather than *vice versa*. From the study of many examples we have learnt that they are hereditary and that they depend on the composition of the atmosphere. Every fear of their propagation by sporidia is superfluous.'

As a student of the smaller fungi, or as

we would say nowadays, as a microbiologist, Fries was a supporter of spontaneous generation ; in so far as he was a student of plant disease, he belonged to the school of the predispositionists the leader of whom was Unger. These held that a fungoid disease is the direct product of the environment. Place any plant in a certain kind of environment, whether of soil or atmosphere, and it inevitably becomes diseased in a characteristic manner, one of the characteristics being the appearance of a particular fungus which has originated from the disordered sap or tissue of the plant. It was the hereditary property—the predisposition—of the plant that it should react in that kind of way to those circumstances. As we freely recognize nowadays, this theory of plant disease has a considerable element of truth in it, for the environment does play a highly significant rôle, in some diseases more so than in others. The modern view has not discounted the disposition of the plant while elevating the status of the parasite.

The position taken up by Fries, which I have just outlined, was the one generally accepted, down to about the middle of the century. From time to time a different view was expressed, for example, that the sporidia which Fries considered to be so unimportant were able to germinate, and it was suggested that they might gain entrance to plants through pores in the leaves or might find their way into soil and be taken up by the plants' roots. There was however no clear evidence on the point. In the main, fungi continued to be described merely in their separate occurrences, without any attempt to follow up their further progress. Developmental studies were the great achievement of the Tulasnes and of de Bary, who were actively laying the foundations of a new mycology a hundred years ago.

The *Selecta Fungorum Carpologia* of the Tulasne brothers appeared in 1861–5 ; de Bary was at the height of his activity at about the same time and his ' Comparative Morphology and Physiology of Fungi, Lichens and Myxomycetes,' which was published in 1866 as Part II of Hofmeister's Handbook, is probably the most notable book in mycological history. These were the pioneer investigations of fungal life-histories. The method of the Tulasnes was to piece together, by careful and methodical study, the different stages of the same fungus. Mistakes, of course, were made, but the great bulk of the work has stood the test of time and, as every mycologist knows, the *Carpologia* has never been surpassed for the accuracy of its observations and probably never equalled for the magnificence of its illustrations. De Bary was essentially a morphologist, but he was also a pioneer in the cultural method of study, and though he became only secondarily a plant pathologist he was sufficiently eminent in that branch to be rightly considered as the father of scientific plant pathology. It was he who first showed, beyond all possibility of doubt, that the spores of a Rust fungus, by germinating on the surface of a suitable plant, produce a mycelium within the tissues of the plant and that this mycelium gives rise in due course to a fresh crop of spores. It was in this way, and in this way only, that the rusted condition of the plant came into being. In emphasizing the necessity for life-history studies, de Bary wrote as follows : ' This is simply the method by which we determine that the apple is the product of development of the apple tree, and that the tree is produced from the apple ; the logic does not change with the size of the objects or with our apparatus and manipulation.'

The work of the Tulasnes, of de Bary and of others who followed their lead established the independent existence of fungi. They were plants in their own right, with life cycles of their own, and they were to be studied along the same lines as were being followed for other plants. The way was now open for advances along a variety of lines, and I shall endeavour to follow up the more important of these.

One of the first fruits of the new method of approach was somewhat unfortunate, though readily understandable in the circumstances. It came about in the following way. The *Carpologia* of the Tulasnes is essentially a demonstration that typical members of the Ascomycetes produce two kinds of sporing structure. They do this at different times and with great regularity. In other words they are pleomorphic. De Bary also followed in the same path when he showed that the Aecidium rust on barberry was a stage in

the life-cycle of the Uredo and Puccinia rusts on cereals, the connection between the latter having been arrived at earlier. It is curious to recall that this association of barberry bushes with rusting of cereals had been suspected by agriculturalists some centuries earlier, and that de Bary himself, in common with other mycologists, had scoffed at the idea. Pleomorphism therefore became the rage in mycology, and this led to some strange results. There was a similar development in bacteriology, Nägeli being the chief protagonist of bacterial mutability. One bacterium changed into another, or into a yeast, or it might be into a mould, and so on. If two fungi appeared in succession at the same place they were considered to be related. Brefeld, who perhaps more than any other man was responsible for the elaboration of methods of studying micro-organisms, tartly summarised this wild phase in the remark—'The only thing that comes out of that sort of work is nonsense and *Penicillium glaucum*.'

The trouble arose from excess of enthusiasm combined with inadequate technique. Pure culture methods are necessary for absolute proof, and some time was required to make these reliable. The nutrient medium had to be sterilised, and ordinary boiling was sometimes successful and sometimes not. It was not until 1876 that Cohn demonstrated the spores of *Bacillus subtilis*, the hay bacillus, and their remarkable capacity for resisting heat. This organism caused much trouble in the earlier work, and it contributed materially to prolonging the life of spontaneous generation. It is worth recalling that John Tyndall, the eminent physicist, became involved in the technique of sterilising liquids, as a sideline to his optical researches, and he had some mixed experiences. Incidentally it was Tyndall who developed the method of discontinuous sterilisation which is still in use. Then there was the problem of protecting a sterilised medium from outside contamination, and this was solved in due course by means of the dry cottonwool plug. It was not until the 1880's that the gelatine or agar plate and the familiar Petri dish came to be adopted, all I may say from Koch's laboratory. Methods of separating organisms in mixed culture were crude and unreliable, and for some time the only known means of purifying a pathogenic bacterium was to pass it through a series of the susceptible host, a laborious and not always reliable process. The pure culture method had its teething troubles in plenty so that one need not be surprised that there was a period of confusion and misdirected effort.

I have already stated that by 1850–60 the time was ripe for applying to fungi the concepts which were then held in relation to other plants. There were two great concepts which took shape in the same decade and which had a profound effect on the further development of biology in general and of mycology in particular.

Darwin's 'Origin of Species' was published in 1859 and everyone knows that this marks an epoch in biological thought and study. Hofmeister's researches which led to his enunciating the theory of alternation of generations were in progress over the years 1849–57 and these likewise mark a new epoch in Botany. The two theories were not independent. Darwin formulated the principle of evolution in biology and Hofmeister's work gave the key to the evolutionary process in plants. It states, in essence, that in all plants from mosses and liverworts upwards there is a regular alternation between a spore-producing (sporophyte) generation and one which produces sexual organs (gametophyte). This immediately linked up a wide range of plants into an ordered series, so that it became possible to visualise relationships and evolutionary trends. An obvious line of enquiry was to see how far the Hofmeister principle could be extended to the lowest group of plants—the Algae and the Fungi. The technical difficulties are greater with these plants and there is furthermore a greater range of types, so that the rate of progress has been correspondingly slow. The work was begun by de Bary and its completion has required at least three-quarters of the century under review.

The alternation of an asexual with a sexual mode of reproduction was soon established for the Phycomycetes, that group of Fungi which has distinctive sex organs and which is generally considered to be nearest to the Algae. A sexual phase was claimed for the Ascomycetes by de

Bary, but it was little more than a guess as he was unable to furnish the proof. This view was opposed by the equally redoubtable Brefeld. It happened that de Bary was correct, but it was not until 1895 that sexuality in the Ascomycetes was actually proved by Harper. The solution was even more difficult to find with the Basidiomycetes, and had to await the first quarter of the current century.

The trouble really was a technical one. As we now know, definite sex organs are much reduced in the higher fungi and in the highest of all they are completely eliminated, so that sexuality is largely or entirely an affair of nuclei. It was not until adequate cytological methods had been developed, i.e. until the time of Strasburger, Van Beneden, Overton, Flemming and others (which was from 1880 onwards) that nuclear phenomena—fusion, division and more particularly reduction division, chromosome behaviour and so on—came to be understood. The real meaning of sexuality was then made clearer, and it was possible also to give a more exact statement of the Hofmeister principle. Alternation was fundamentally one of a haploid generation with *n* chromosomes in the nucleus with a diploid generation with *2n* chromosomes. The beacon lights in this cycle were the stages of nuclear fusion and reduction division, and by determining where those events took place the cycles of various groups could be orientated in relation to each other. By nuclear studies it was possible to overcome the confusion which had arisen from the multiplicity of sporulating mechanisms in fungi.

As I have already mentioned the elucidation of the cyclic process in development was accomplished last of all in the Basidiomycetes. An important lead to the study of these was given when Blakeslee discovered the phenomenon of heterothallism in the Mucorineae. This was in 1904. By 1920 Kniep and others had established heterothallism for the Ustilagineae and within the next 10 years the same had been shown to apply to the Rusts by Craigie, to higher Basidiomycetes by Buller and coworkers and to Ascomycetes by Dodge (for Neurospora), by Winge and by Lindegren (for yeasts), and so on. I shall have to come back to the subject of heterothallism in another connection ; it merely remains to say here that Blakeslee's original discovery led to the clearing up of life cycles for fungi as a whole, and though there is considerable variety of detail in the various groups it is now established that the Fungi (as well as the Algae) conform with the principle of Hofmeister, which thus becomes one of the great generalisations, perhaps the greatest, of botanical science.

The study of fungi from the evolutionary standpoint arises naturally from the foregoing and much has been written on this intricate topic. The tendency of the earlier mycologists, e.g. de Bary, was to make the Phycomycetes the starting point of the group, Ascomycetes and Basidiomycetes evolving from them by progressive loss of sexual apparatus. It is more usual now to hold that fungi have branched off at a number of points in the algal series, in other words that the fungi are polyphyletic in their origin. Within the fungi themselves it is possible to suggest evolutionary trends, but there is much diversity of opinion on particulars. I shall leave it at that.

I have already touched upon the taxonomic position as it was at the time of Fries. The studies relating to alternation of generations also had an effect on taxonomy but any attempt at detail would be impossible here. Some of the groups have remained substantially as they were left by Fries, apart from the splitting up of some of his very large genera, such as Agaricus. Others were more difficult to place and have therefore had a more chequered history. This applies to many of the microfungi and notably to the important parasitic families of Smut and Rust fungi. By the time the second edition of de Bary's classical book appeared (1884) one finds that the main lines of classification are very much in accord with modern views. De Bary makes the Ascomycetes the central group ; he speaks of an Ascomycete series, to which he attempts to relate the others, in conformity with his monophyletic conception of fungal evolution. The difference between his classification and any modern one is more apparent than real, being largely a matter of the use of words. On a smaller scale, as for example within the family of the

Rust fungi, de Bary was led to a grouping very different from the one familiar to us, but this was because the necessary cytological knowledge did not yet exist. Even so, one cannot read de Bary without wonder at his intuitive grasp, for many of the later developments are foreshadowed in his writings. I shall conclude this short review of the advance of fungal taxonomy by remarking on a striking, I might say a terrifying, feature of it, and that is the enormous number of species which have been recorded and named. There are more than 80,000 in the pages of Saccardo, and many have been added since. I shall come back later to the question of fungal species.

There is another line which has led to considerable developments and which I shall now follow up. The systematic mycologist, though rather perturbed by the evidence for pleomorphism to which I have referred, still continued to collect fungi in nature and to describe and name them as he found them. There was no practicable alternative to this way of working. Obviously any specific description and naming would be meaningless if the organism, under more or less the same conditions, showed any tendency to diverge materially from the accepted type. The bias of the systematist was therefore towards constancy in fungal species and a similar view was stated by Cohn for bacteria, in spite of Pasteur's work on attenuation of virulence in some which caused disease. As time went on—and this brings us to about the beginning of the present century—it began to be realised that even pure cultures, i.e. ones in which there was certainly no gross admixture of different kinds of organisms, might occasionally change their properties. There might be an increase but more often a decrease in pathogenicity, or a change in some biochemical property or even in the morphology of the organism. By this time the science of genetics was being actively developed for the higher plant groups and the question immediately arose as to whether these authentic sudden changes in bacteria and fungi were comparable to the mutations or to the Mendelian segregations which had been demonstrated elsewhere. The phenomenon in fungi, to which the non-committal name of ' saltation ' or jumping was given, was important and in fact distinctly troublesome to the experimental mycologist as it meant that his cultures might no longer be suitable for his particular purposes. It also had significance for the detailed classification of fungi, since the changes occurring in saltation might overstep the accepted specific or even generic limits. It appeared to many therefore that the specific limits drawn for many fungi had been too narrow, whence in part the enormous number of species which had been listed. On the other hand there were some who maintained that saltant cultures were merely artifacts, that they were products of laboratory manufacture and that they would not arise under the natural conditions prevailing in the open. Such a view which rather suggests that the laws of nature can be put in abeyance by man's handiwork does not seem to be very profound, but the convincing argument against it has been provided by experience.

As maintained in culture, an ever increasing number of fungal species have been shown to throw off variant forms, some of which are relatively static, others mobile, all diverging somewhat from the original type. One says that the species exists in the form of a cluster of strains, and the range of characters shown by this cluster is wider than that of the original strain. Now precisely the same conclusion applies to fungi in their natural state. Where any species has been extensively studied in its natural occurrence it has almost invariably been shown to consist of a group of strains. The most startling illustration of this complexity is perhaps given by the rust fungus *Puccinia graminis* of which over 100 strains are known, all of them occurring naturally. In this example strains differ only slightly from each other in morphological features, the important differences being physiological, whence the term ' biological race.' With other species morphological as well as physiological differences may also be shown, as in the strains of *Penicillium notatum* which have been studied in connection with the production of penicillin. This feature of fungi is so common as to be almost universal.

Further advances in the study of fungal variation have resulted from Blakeslee's

discovery of heterothallism, because it led to the development of a new experimental method. Haploid races of different sex could now be separated out and mated again in various combinations. The characters of the progeny could then be studied. This is precisely the technique which is employed in the study of inheritance—Mendel's method—and we are therefore now witnessing the growth of a fungal genetics which has already achieved important theoretical and practical results. There is even on the way a cytogenetics of fungi with sex-linkage, lethal factors, crossing over, etc., in spite of the difficulties arising from the smallness of their nuclei. Some geneticists in fact claim that fungi are very suitable subjects for their purpose, inasmuch as they grow quickly, can readily be kept under environmental control, occupy little space, so that research can be carried out very cheaply. The outcome of all this has been the proof that fungi are subject to the same genetical laws as are other plants and that within the species they show a similar type of variability which rests on genetic factors. By a process of crossing, new groupings of characters can be produced, more or less at will. Hence the possibility, and in fact the actuality, of improved races of fungi for economic purposes, as has been recently shown with strains of yeast for brewing and of Penicillium for penicillin production. There are also dangerous possibilities, as in the appearance of more virulent strains of pathogenic organisms, either by mutation or by natural hybridisation, or according to some workers, by a chemical process of adaptation. Examples which illustrate this danger are numerous and continue to increase ; two such of recent origin are the strains of the potato blight fungus which are able to attack varieties of the host which were hitherto immune and the strains of Streptococcus which are resistant to penicillin.

So far I have been concerned mainly with morphological and cognate aspects, but I must now turn to physiology. To many this is the most important and interesting feature of the group. If it be true, as I have tried to show, that our knowledge of fungal structures and relationships is almost entirely a product of the past hundred years, the same is true *à fortiori* of our knowledge of fungal physiology. Clearly nothing of value could have existed prior to the fundamental morphological work of which I have spoken, nor indeed until some 20–30 years later, i.e. until the technique of pure culture methods had been developed. From then onwards physiological research has continued to expand, and at no time has it been more active than it is to-day.

The advantages which fungi offer for physiological study are many, and I mentioned some of them when speaking of fungal genetics. As compared with higher plants, fungi have a much simpler structure, and this applies particularly to such types as moulds, yeasts, etc., which have been favourite subjects of physiological research. Complications arising from compactness of structure or from differentiation of tissues are thus largely avoided. Many of them can be grown on very simple media which are reproducible at will, and therefore the starting point of the reaction can be clearly defined. Furthermore the conditions prevailing outside and inside the culture can be adjusted with considerable ease and exactness. Fungi (and bacteria) are thus ideal plants for biochemical studies and an enormous literature has developed in this field.

While it may be said that most plant physiological problems, with the notable exception of photosynthesis, can be studied in the Fungi, there is one process to the study of which they particularly lend themselves, viz, respiration. Weight for weight, fungi and bacteria are among the most actively respiring organisms known, in conformity with their function in nature as agents for the breaking down of organic material. Furthermore they offer a greater range of respiratory processes than do other organisms—from aerobic to strictly anaerobic in addition to special types, as in the conversion of ammonium salts to nitrate, sulphide to sulphate, ferrous salt to ferric. Particular interest and importance attaches to the capacity of some micro-organisms to 'fix' atmospheric nitrogen, thereby making this element available for use by higher plants. All these developments took shape in the latter part of the nineteenth century, and work in these fields still continues.

From the general biological point of

view special interest attaches to the process of alcoholic fermentation. The modern view of aerobic respiration, which is the normal process in animals and plants, is that it is preceded by an anaerobic phase and that the latter is essentially the same as the reaction which occurs when sugar is fermented to alcohol by the agency of yeast fungi. Quite apart therefore from their industrial importance, the yeasts present a problem of the greatest biological interest, and it is not surprising therefore that they are among the most investigated of fungi from the biochemical standpoint. The work of Harden may be cited in this connection.

The science of biochemistry immediately conjures up the science of enzymology which also is a development of the last 50 years. Enzymes are present in all living organisms, in which they catalyse a great variety of reactions, both of an up-grade and a down-grade character. Probably no other group of plants or animals compares with the fungi and bacteria in the variety of enzymes which they produce and in the ease with which those enzymes can be studied. They have become, one might say, the happy hunting ground of the enzymologist.

I shall refer only briefly to the wide field of industrial microbiology which has arisen from the biochemical developments just mentioned. The oldest example of an industrial process which depends on a micro-organism is, as everyone knows, alcohol fermentation. The further fermentation of alcohol to vinegar also much antedates the scientific period. Within the century under review many new and important biological processes of similar kind have been developed. Each of these involved the finding of an organism which was specifically adapted to carrying out the particular reaction desired. The cultural conditions which led to a maximum yield of the product had next to be worked out. Care must be taken to circumvent any tendency of the organism to lose its activity ; conversely it has been possible to obtain more active strains by encouraging mutation or by hybridizing. In this way the industrial fermentation of sugar to citric acid has been developed to the extent that it has replaced the old method of extraction from lemon. The production of acetone and glycerol by fermentation processes during the first world war illustrate the same method.

In the examples just quoted, the manufactured compound is an intermediate respiratory product of the carbohydrate supplied to the organisms. There are in addition important fungal syntheses, of which two earlier examples were Taka Diastase from a species of Aspergillus and Vitamin B from yeast. The most spectacular success in this field has, however, been the preparation from fungi of valuable germicidal substances, like penicillin, streptomycin and others. The discovery of penicillin led immediately to what an American writer has aptly described as a ' mycological gold rush ' in which the whole range of fungi was feverishly explored for new types of ' antibiotics.' The finds, though of the greatest value, have been relatively few, but it does not follow that this field of enquiry has been exhausted. The more thorough investigations which are now going on may lead to the discovery of other valuable compounds, and incidentally they throw new light on the processes of fungal metabolism.

There are two lines of physiological advancement which are associated with the names of Raulin and of Wildiers and which call for notice. These relate in the first instance to the ingredients which must be present in a culture medium if the latter is to be satisfactory for growth.

Raulin as early as 1869 showed that over and above the standard mineral elements normally supplied to culture solutions, traces of zinc salts were required for the proper growth of the fungus *Aspergillus niger*. This conclusion was refuted by no less an authority than Pfeffer who maintained that any effect of zinc was merely stimulative and not strictly essential. The subject ceased to attract attention until comparatively recently, by which time the same problem had arisen in the mineral nutrition of higher plants. It is now definitely established for a wide range of plants that growth will not take place satisfactorily in the complete absence of certain mineral elements. The number of these is surprisingly large, including zinc, boron, manganese, molybdenum, copper and others. The merest trace may be sufficient to give the

full effect, so that in work of this description extreme precautions have to be adopted to remove traces of impurities from the materials or apparatus used. Hence the inconclusive nature of much of the earlier work. The sensitiveness of *Aspergillus niger* to traces of zinc illustrates this point. By means of this fungus one can detect and measure amounts of zinc which are quite beyond the range of chemical test. This work has both practical and fundamental interests. It has revealed the existence of highly significant 'deficiency diseases' in many of our crop plants and has shown the way to their treatment. From the fundamental standpoint it is now clear that trace elements play an essential part in metabolic processes, most probably by their effect in activating enzyme systems.

Wildiers first showed in 1901 that certain yeasts would not grow on purely synthetic media unless there was added to the culture a small quantity of a naturally occurring product, such as peptone, various proteins, extract of yeast cells, etc. To this substance he applied the somewhat fanciful name of 'Bios.' The Bios theory led to an extremely controversial literature but has at length taken a coherent form. It is now established that a variety of more or less complex organic substances is required for the growth of micro-organisms, substances such as biotin, aneurin, inositol, etc., and that small traces of these are sufficient for normal growth. There is considerable variation among micro-organisms in their capacity to produce some or all of these substances, and it was this difference in behaviour which led to the confused state of the literature on this subject in its earlier stages. The relation of fungi and bacteria to the 'Bios' type of substance shows many resemblances to the vitamin story in animal nutrition, and in fact some members of the Bios complex are closely related to vitamin B. Several interesting associative effects have been demonstrated in which two fungi are able to grow on a medium on which neither can be grown separately. The one fungus is able to synthesize the particular Bios constituent which the other requires. The exploration of these relationships is very active at the present time.

As a last illustration of the course of advancement I propose to consider a branch of mycology which has both physiological and morphological aspects. This concerns the influence of micro-organisms on other organisms. I have already touched upon this subject, when speaking of antibiotics where the action is antagonistic and of the Bios phenomenon where two fungi may aid each other. Mutualistic actions of fungi and bacteria, both in laboratory cultures and in their natural habitats, have often been observed and there is an expanding literature on this topic. Microbiological antagonism, as it is usually called, has a considerable influence on the presence or frequency of micro-organisms in soils and as such has a determining effect on the character of the soil flora. This line of study is of quite recent origin; its importance in relation to the prevalence of root diseases of plants is obvious.

It is, however, the relation of fungi to other groups of plants that I wish more particularly to consider. According as the effect on the other plant is beneficial or the reverse, one speaks of symbiosis or of parasitism. In text-books these are usually treated separately, though in fact they have much in common both physiologically and morphologically. The one may be converted into the other by a comparatively slight change of circumstances and in a whole class of plant diseases a symbiotic phase can be distinguished before actual damage to the host plant occurs. I shall follow the usual practice and sketch their development separately.

The symbiotic nature of lichens was first suggested by Schwendener in 1868 and the proof that a lichen is produced by union of the appropriate fungus and alga was given a few years later. The physiology of this group has been strangely neglected for they seem to offer many problems of physiological interest. In 1885 Frank discovered the presence of fungi in the roots of trees and subsequent research has shown that this association of fungi with the underground parts of plants ('mycorhiza') is widely spread, especially in forest trees, in heath-inhabiting plants and in special families such as the orchids. Of the same type are the bacterial galls normally present on the roots of certain

plants, in particular of the leguminous family. The morphology of these structures is now fairly well known but it cannot be said that their physiological relationships are at all clearly understood. It is generally accepted that there is a mutually beneficial action, the fungus being thought to obtain carbonaceous food from the higher plant and the latter its mineral salts by the agency of the fungus. Much of this subject is still in the controversial stage. The interrelation between the leguminous plant and its root nodules has been much investigated in view of the importance to agriculture of the process of nitrogen fixation which takes place in the nodules but here also there are many problems which await solution.

The fact that fungi are the chief causal agents of plant disease is one of the main reasons of their economic interest. The science of plant pathology which is largely, though not wholly, concerned with the attacks of fungi on higher plants came into being about one hundred years ago, as I have mentioned in the early part of this address. The severe epidemics of potato blight which occurred in Western Europe at about that time gave a powerful stimulus to plant disease study. Notable pioneers, in addition to de Bary, were Kühn in Germany, Berkeley in England, Millardet in France, Woronin in Russia, Burrill in the United States and many more. This branch of applied mycology is outstanding in at least one respect, that it has been specially fostered by governments with the object of reducing the annual losses caused by diseases of crop plants. A period of active expansion began about 50 years ago and has steadily continued until the present day. In this the lead has undoubtedly been taken by the United States where there are strongly staffed departments of plant pathology in many of the universities, and plant pathological divisions in the United States Department of Agriculture and in all the State Colleges. As a result a large proportion of the significant literature on plant disease now comes from that country. In Britain the subject is included in the curriculum of a number of universities and it figures prominently in the activities of the chain of national agricultural and horticultural research stations. The time is long past when the needs of the plant industry could be met by a staff member of one of the national herbaria. We have now research stations for the various types of crops—for fruit, glass-house crops, vegetables, cereals and forage crops, and overseas for rubber, cotton, tea, cocoa and so on. This is the age of specialisation.

Time will not allow me to sketch even the broad lines of advancement in this subject. I shall merely say that by now the vast array of economically important plant diseases has been studied in considerable detail ; we now know a great deal about the life-histories of the important parasites and how the course of various diseases is influenced by the environment, and we have learnt how to reduce their ravages in a variety of ways, by methods of improved culture, by the use of disinfecting or protecting chemical substances, or by the breeding of resistant or immune varieties of plants. But the end is not yet in sight. Where some control of a disease has been effected, there still is the urge to find a better, and there are many important disease problems which await a practicable solution.

The review which I have given you is a record of progress and development along many lines ; the mass of knowledge is now formidable and beyond the scope of any one man. I should be sorry however if the contemplation of these achievements led any of you to think that the work is finished. It may be that in certain branches the subject has reached a limiting point. Morphology and classification may have become for the time being more or less static but this is far from true of physiology in which, from the nature of things, there can be no finality. Looking ahead, one might guess that the future of mycology and of biology generally will lie in physiological advancement, and this in turn may have an impact upon morphology. The physiology of the future will no doubt follow the trend which is already on the way, that is, it will become more and more biochemical. Many of us, who are not too firmly grounded in biochemistry, may feel uneasy at this prospect, may feel that we are handing away our subject to strangers. But I do not think that we should take too humble

a view of ourselves. After all it is the biologist who poses the exact problem for the biochemist. He says in effect that there is such and such a process taking place and that in all probability there is a chemical substance concerned in it. What is the substance and what are its properties? Granted an answer, the problem again reverts to the biologist, whose function it is to apply the new information to a deeper understanding of the biological process.

This co-operation will be made easier and more fruitful if biological training is orientated more in a physico-chemical direction, especially in view of the rapid advances which are taking place these days in fundamental aspects of physics and chemistry. Considered in this light, the recent change in the schools whereby a certain amount of biological instruction has been substituted for further training in physics and chemistry is unfortunate. It is all to the good that the ordinary citizen should be equipped with some general ideas on biological matters ; to the student who aims at making a career in biological research, the change is a retrograde one, for it cuts him off prematurely from the kind of training which, highly desirable as it has been in the past, appears likely to be essential in the future.

MID-CENTURY—RETROSPECT AND PROSPECT

Address by

SIR HECTOR HETHERINGTON, K.B.E.

PRESIDENT OF SECTION L

LIKE most Vice-Chancellors, I am well accustomed to do as my Professors tell me. So when my former colleague Dr. Hindle explained that at this meeting of 1951 it would be appropriate for me to try to review the century's changes in the British educational scene, I took my orders. I have this measure of dubious comfort. I have lived through more than half the period, and all my working life in different parts of these islands I have been involved in the educational process. Perhaps this basis of personal experience a little compensates for my lack of specialist learning in this field. Even so, I am well aware that this address will be all too meagre an acknowledgment of the great honour which the Association has done me in calling me to the Presidency of Section L, and for which I desire to express my gratitude.

You will recognise, of course, that except in diagrammatic form, my task is impossible of fulfilment. Our educational journey is the reflexion in one particular medium of the whole complex movement not of one society only, but of the three components of Great Britain : and that in a century of unexampled change. The story is full of drama, and of the accidents which affect the course of history. And yet, as the record runs, I think there is visible a fairly consistent pattern. It is that pattern which I shall try to elucidate. The clue to it lies not only in the developing logic of the educational system itself, but in the transformation of a whole social situation, so that the ideas and principles which have been at work in education have been nearly related to those which have found parallel expression in other phases of our endeavour.

I mean, therefore, first to try to describe this broad social movement : then, in the most summary fashion, to exhibit the corresponding development of our educational policies : and finally to say where that has brought us, and what tasks and opportunities appear to lie ahead. You will observe, I hope, that I follow the architecture of the Scottish sermon.

I. First, then, can we discern any broad philosophical intention which has characterised the general political and social achievement of our country over one hundred years? I do not think this an absurd question, though that profession may mark me down as an 'unscientific' historian. Still, my account of the matter is this—that there has been at work a principle or impulse which, following Dr. Gilbert Murray, I shall call 'liberality.' You may think it odd that I should so describe a movement which in many spheres has superseded or qualified individual initiative and responsibility by measures of collective action and public control. The defence must come as I go along. I am not talking of Liberalism or indeed of an outlook which is the exclusive property of any political party. I am concerned with a motive which has been strongly felt by all parties, and which over a considerable area of public affairs, has induced consent to an almost identical programme. You may, if you choose, describe as Socialism the practical measures involved, like Sir William Harcourt in his famous dictum of 1895 that we are all Socialists nowadays. But the operative force has been not Socialist theory, as the Continent understands that term, but the apparent efficacy of specific modes of collective action in solving the practical problems

which emerged in the attempt, in this direction or in that, to apply this principle of liberality. Of course I am not denying the influence of Socialist thinkers, which has been very great. But the thinkers who have mattered most were not the theorists like Marx, but those like Chadwick and Sidney Webb or William Beveridge—only one of them a Socialist—who set themselves to analyse a particular situation, and devise a policy to produce the desired result.

Broadly I mean by 'liberality' the impulse towards a wider distribution both of political power and of economic and social opportunity: towards, therefore, greater equality of status among different members of a community, and greater personal participation in and responsibility for its affairs. Another name for it is democracy, in the more or less characteristically British version of that idea. It set to work, in mid-century, in unpropitious circumstance. It was by no means new in our history. But the swift impact of the Industrial Revolution upon a society in nowise ready to cope with such a change had wrought a calamitous distortion of the old ways of life: and by the early nineteenth century, had produced a polity beset by extremes of wealth and power and of misery and helpless poverty. Britain was divided, bitterly divided, as it has rarely been. By the 1850's the rallying process had begun: and the hundred years since then have transformed our institutions and brought again a measure of reconciliation.

That this has happened with relatively little fundamental controversy—with controversy less about objectives than about the means and pace of the movement thereto—implies two conditions. The first is that beneath all differences and strong enough to hold them in check, there has been a common stock of moral or ethical ideas. Men have been well agreed as to the kind of society they ought to want, and as to the relations and attitudes which ought to exist between its members. The second is that they have been living in the same world, and have been aware of the same forces as shaping the conditions of their lives. The first, the stock of ethical ideas, is still familiar, though subject in recent years to much dilution and to a challenge of growing severity. It derives from the roots of our Western civilisation, in classical humanism and especially in the teaching of the Christian Church. The central matter there is an assertion about the nature of man, and about the divine purpose and plan for his redemption, from which it follows that every man is himself a bearer of values, never to be treated by others as mere means to their own ends, entitled always to a chance in life, and due both to give and to receive not justice only, but mercy and kindness. That, I am sure, is the essence and the source of liberality and democracy. And with this has been linked a confused empirical belief, neither classical nor Christian, but not without warrant in certain elements in both traditions, that most men are decent reasonable people, who may be trusted, in the end, to see and respect the truth, and to take the right decisions so that the nature of things has a bias towards righteousness, and the good is sure of ultimate triumph. Our present mood is not so confident. But until the other day, and for far longer than a hundred years, this, or something like it, though for a season submerged, has been the accepted ethical stock-in-trade of the British people. And it has this much to say for itself that it affirms the spiritual nature of man, and gives him a scheme of values to live by.

As to the second condition, there is no mistaking the main factor which has affected the outward conditions of human life. Nothing compares in importance with the coming to full maturity of natural science and of the technological economy which has resulted therefrom. That is the key to the whole material development of our time. It too had been long preparing. The later seventeenth and eighteenth centuries will rank among the most illustrious periods of British science: and early in the nineteenth, the work of Dalton, Davy and Faraday had begun to produce revolutionary effects both on the technology of the day, and on the fundamental concepts of science itself. By 1850, therefore, the stage was fairly set: the full dynamic of the new Industrialism on the one side, and on the other the growing perception that these energies left to themselves were producing a society harassed and troubled by the sufferings of many of

its members. Thus the impulse to liberality and the growing range and dominion of applied science, came consciously to confront one another, and to set for the succeeding age, the problem of so directing our political and economic action that the new resources might become the instrument of a better and freer life for the ordinary man. The Communist Manifesto of 1849 was one expression of the confronting and one resolution of the conflict. But there were other and better answers, evolved by other minds, who saw the problem not in terms of economics only, but as Robert Owen had done nearly fifty years before, in terms also of politics, of social manners and of education.

I do not repeat to you the history in detail. It is familiar enough. The second half of the nineteenth century brought still greater developments in science and technology : and in the fifty years of our own century the pace has been headlong. Our scientists have immeasurably extended the time and space scales of our world, have given us insights and resources undreamt of a hundred years ago and in consequence have changed the whole fashion of our action and of our thinking. They have added to our years, to our numbers, to our productivity and to our comfort. They have removed much suffering and the direst poverty. But more and more they have crowded us into great cities : have made us subject in work and in play to mass-organisation and pressures : have made us more interdependent, have multiplied our points of strain, and immensely increased the perils of any failure in our mutual adjustments. And on the other side, and at much the same tempo, has come the transformation of our economic and political systems. First, for fifty years the gradual extension of the franchise, the growing power of working-class organisations, in Parliament and local government, in Trade Union activity, in friendly societies and in the Co-operative movement. Then at the turn of the century, inaugurated by the Liberal Government of 1906, an impulse of astonishing energy towards what we now call the welfare state, retarded for a time by the first war, and the long ensuing economic winter, but in the end deriving from that experience an irresistible momentum. Successive changes not only in the franchise but in electoral law and in the composition of the parties have patently transferred political power to the masses of the people : and it has been used increasingly not only to regulate working conditions but to distribute the product of industry and to establish an economy which takes as its primary objective the raising of the standard of life of the ordinary citizen.

All this is a thrice-told tale. As we look back over its course, it is easy to recognise how, given the confrontation of 1851, such a development as this has followed. There was no inevitability about it. But it has a logic of its own : though not that which was most confidently announced. Nor, of course, does its logic in the least degree establish that every successive stage has been wisely taken, or that the final result is a permanent or even a viable economy. A less comfortable *dénouement* is altogether possible. We have transformed an established scheme of things and in so doing have displaced or weakened many of the harsher motives and sanctions on which it relied to preserve its order and get its work accomplished. That process is far advanced. But we have not yet brought into effective play the higher motives on which we must now rely : and until we are able to do so, we face the risk of failure. There is a challenge here, of which we must be thoroughly aware. I make no prediction at all. Yet I do not doubt that the experience of this hundred years has disclosed some of the conditions which a developing civilisation must seek to fulfil : and that we have not mistaken the way to a more humane society in which men can be more themselves and more content in their service. And the proof of it and one solid attainment is that in considerable measure the deep divisions of our society have been overcome. I know the qualifications which remain. But it is true in a sense in which it was not true in 1851, that we are one people.

II (A). I turn now to mark the corresponding course of our action in education. It, too, has its background. Every part of these islands has a long educational history—schools and colleges founded

throughout the centuries, some in splendour by Kings and noblemen and great ecclesiastics, but most—the grammar schools—in simpler homes by the piety of private benefactors and by the efforts of corporate charities. But in England at least, that stream touched only the relatively few—the sons, though not the daughters, of the wealthy or of the rising middle-class. For the children of the working-class there was little, and the standard of literacy everywhere was extremely low. In education, indeed, there was the same class division as in other things.[1]

The first struggle, inevitably, was over the provision of primary education. The lack of it was a challenge both to the bearers of that impulse of liberality of which I have spoken, and to others who saw the imperative need of the new industries of Britain for a better-instructed working population. As long ago as 1789, the French Revolution had injected into British thinking the idea of a state system of education, universal, compulsory and free. The idea was never lost. But it was slow to come. By 1833, Parliament had so far asserted the concern of the State as to sanction small building grants to the denominational societies which alone provided the country's primary schools and maintained them partly by charitable subscriptions and partly from fees. 1839 saw the first beginnings of a State Department and brought a great man, Kay-Shuttleworth, on the scene. The building grants were rapidly increased. But there was no yielding on the issue of principle. For the governing mind, the grants were an admission of public concern, not of public responsibility. The State might act as a secondary and supporting, but not as an initiating, agency: and its action could extend no further than the bare elements of education. The first of these principles was not breached until 1870: the second, though subject to gradual erosion in the following thirty years, was not decisively breached till 1902, and not thoroughly overcome for forty years thereafter.

That is a long time, though the reasons are evident. One was the dreadful confusion of local government, so that there was no available alternative to the unacceptable basis of direct administration by the central government. But the deeper cause was the resistance of the denominations to any invasion of their established sovereignty over primary education. It was not merely a 'vested interest': its more respectable ground, held by others than the religious, and not disproved by the experience of wholly secular systems elsewhere, was a belief that to entrust education to the State, to encourage learning with little care for piety, was to imperil both the individual soul and the national peace. I suppose the fires of that controversy have burned themselves nearly out. Over a long period we have gradually come to an accommodation. But the issue went deep into English life and still remains. The delay was costly—to the influence of the Churches as well as to the schools. But it may be that we have saved something that was worth preserving.

On that matter, the Act of 1870 broke the monopoly. The State, through a new Local Authority, the School Board, became a providing agency in its own right: not the sole providing agency and still auxiliary to the voluntary society, but entitled to act directly wherever the voluntary provision was inadequate. At last Parliament had ordained that there should be a school accessible to every child, though not yet that every parent must send his child to school, or that he

[1] Throughout this paper, for brevity's sake, I have written more or less in terms of English experience, and have made few references to Scotland, Wales or Ireland. There is no great harm. The problems are basically the same: and the answers come, on a broadly similar time-table, in much the same order. Indeed from the establishment of the Committee of Council in England in 1839, the Minutes of that Committee controlled education in Scotland and Wales until the Scottish Education Act of 1872. But there are significant differences. None of the other countries knew so severe a class division in education as England. In Scotland, *e.g.*, as long ago as 1560 the First Book of Discipline of the Reformers prescribed that every parish should have a school, where the children of all, rich and poor, were to resort 'so that the Commonwealth may have some comfort of them.' The prescription was slow to be fulfilled: the actual provision in Scotland was made hardly more rapidly than in England. But the declaration, and particularly the Presbyterian and democratic character of the Reformed Scottish Church, deeply affected the spirit and government of Scottish education.

could do so free of charge. Those further steps came, the first in 1880 when the School Boards were required to impose the rule of compulsory schooling : the second in 1891 when fees were abolished in all elementary schools.

The end of the century, therefore, saw the completion, at least in outline, of a public system of elementary schooling, the provision still shared between State and Church, but with the clear assertion of the State's overriding concern to see to the adequacy of the service. An Act of 1889 had also established, curiously not under the School Boards, but under the new County Councils, the beginnings of a system of technical education, extending beyond the age of compulsory schooling. And at the other end of the scale, there had been accomplished the remarkable and momentous development of the new Universities of England, Wales and Ireland. Owens College opened in Manchester in 1851 : the origin of a University which within fifty years was visibly on the way to establishing its present title to keep company with the best anywhere in the world. Birmingham, Liverpool, Leeds, Sheffield, Bristol, Wales, the Queen's Colleges in Ireland and the smaller English Colleges, some of them now full Universities, were all in the first energies of vigorous youth. Some had been created by the ' extension ' efforts of Oxford and Cambridge : others primarily to provide for the growing scientific needs of local industries. But the real foundation of all alike was the determination of the leaders of these cities to provide right in the heart of their own communities and easily accessible to their youth, centres of the highest learning of their time. They gave an irresistible impulse everywhere to the higher education of women. They have succeeded beyond expectation : and the signs are that greater achievements still are within their reach. Even as it is, the record is remarkable. We must take it as read : but we may be certain that without their service, neither our economic nor our educational system could have fulfilled its tasks ; indeed the Britain of the present day is inconceivable.

The task of the next fifty years was to occupy the territory between primary school and University, to give to secondary education not merely an indispensable augmentation, but the full recognition in the national system which had been won so slowly for the primary schools. Wales and Scotland were ahead of England, where the ambiguities were not resolved until after the Bryce Commission, the political courage of Mr. Balfour and the driving power of Sir Robert Morant produced the Act of 1902—by far the most important of our education statutes between 1870 and 1944, and perhaps the most important in our educational history. The Act had its defects, and gave rise to some unexpected results. It did not bring about an articulated national system of education. But for the first time it looked toward such a system and opened the way. It touched every element in our apparatus. But its main decision was to base education on the County Councils and to give them the power and the financial encouragement to prescribe the standards of all Elementary Schools, provided or non-provided, and to establish or to aid Secondary Schools. The response was immediate,—a rapid increase in the number of Secondary Schools, in the number of their pupils and in the level of their achievement, with the further consequence of a rising demand for more of the same sort of thing.[1] By the second decade in spite of the war or because of it, we were canvassing a policy of ' secondary education for all.' The Fisher Act of 1918, though killed by the frost of the later 20's, brought the prospect a little nearer : and twenty-six years later, in the Butler Act, it came to statutory fulfilment.

This great and necessary enlargement of the public provision of secondary education has not impaired, seems indeed so far only to have helped, the older system of the boarding schools. Many of the grammar schools have been incorporated into the public system, some completely, others still as independent elements but in receipt of public aid. But, in the main, the residential schools stand by themselves—still sustained by endowments and substantial fees, and drawing their entrants almost wholly from the private

[1] It is calculated that within 35 years from the Act of 1902, the proportion of children entering upon a secondary school course had increased sevenfold. Even so there was leeway to make up.

enterprise preparatory schools. The threat to their existence comes not from the public system, with which, indeed, good working relations have been established, with beneficial effects upon the standards of both, but from the narrower margins left to the middle-class by the economic and fiscal conditions of our time.

This same half-century has seen the full emergence of two remarkable voluntary activities, detached from the official system, though as a rule with its strong backing and encouragement. One is the Youth Movement—the Scouts, Guides, Guilds, Brigades, Youth Clubs and the like which have enlivened the leisure of thousands of young people and turned it to good educational purpose. The other is the Adult Education movement, dating in its present form from the first years of the century, and reviving again, though this time especially in the field of liberal studies, a movement which had been of great effect nearly a hundred years before.

We may therefore believe that so far as legislation goes, the picture is reasonably complete. We have statutory warrant to do more than we are likely to be able to do for some time to come. We have indeed no State-system of education, no single clear-cut organic plan. But we have a harmony of diversities ; and we have defined and provided for the essential requirements of a national scheme. The basic principle is universal full-time and effective schooling from 5 to 15—as soon as may be, to 16, with, though not yet realised, compulsory part-time schooling until 18, and voluntary attendance before and after the ages of obligation. And within that scheme is enshrined the principle of a genuine secondary education for all. Not the same kind of education for all : for that would be wrong for some ; but for all, education in a good school, calculated through the disciplines appropriate to the interests and aptitudes of the pupil to prepare for the duties and privileges of adult citizenship. So it comes that at length, partly by new creation, partly by the adaptation of older foundations, and partly alas ! only in dream, we have made or are making the whole wide panoply from nursery school to University, primary, secondary, and special schools, colleges and institutes, general or particular, vocational or liberal, full-time or part-time, day or evening, urban or rural, all with their services for health and meals, libraries, art and recreation. We have seen to it that a handsome provision of scholarships and grants facilitates movement across or upwards between the different elements. And beyond, there is the expanding apparatus of further and adult education where a man may pursue his private interest until he wearies of it or dies. It is not all there yet, far from it—there are still avoidable losses of talent and opportunity—and some of what there is is not very good. Man power is scarce, with no sign of betterment. We need not exaggerate our actual achievement or underestimate the difference between plan and fact. But neither need we doubt that we have accomplished something of a revolution. The educational problem is now posed in its true range and dimensions. We have come to think not merely of adding on piece to piece, but of the whole as a single scheme. We have set aside the notion that there are two systems of education—primary education plus a little bit of an extra ending at 14 for all except the better-to-do and the clever : and then secondary education for the minority. If all children are to be within the care of the educational system until 18 years of age, the whole provision from beginning to end takes a different meaning and opens quite new possibilities. And it brings this hopeful corollary. As in the field of economic and political action we have diminished the severity of our class divisions, so, perhaps more decisively, we are moving in the field of education. Differences, of course, remain, and are likely to remain. But the barriers are lower, communications easier, and interchange more equal and free. And that is of vital consequence.

II (B). From these matters of organisation, I come to say a word on the more strictly pedagogical ideas which have inspired or guided our educational practice. I find it hard to put them in systematic form. One reason, I suppose, is that education is an art : and like all the arts is dependent upon the intuitions of the creative genius. But before these insights can pass into current practice, they

have to be interpreted and developed in the light both of educational procedures and of the best available knowledge of the related sciences of physiology, psychology and sociology. Hence our teaching is better not only because of Pestalozzi and Froebel and Herbart, but because we know more of the processes of growth and fatigue, of learning and memorising, of the measurement of different aptitudes and forms of intelligence, of the nature of the emotions, and of all that goes by the name of the unconscious mind. That mediation marks our debt to writers like Graham Wallas and Percy Nunn, MacDougall and Myers, Spearman and Burt and Godfrey Thomson. A second source has been the long accumulated empirical wisdom of the British tradition deriving from and revivified by the great practitioners,—schoolmasters, dons and even administrators. One or two, like Newman, have left a classic text : a few, like Matthew Arnold and Michael Sadler, have joined comparative studies to the inspiration of a winged word. But most, like Stow in Scotland and Arnold of Rugby, Thring, Sanderson, Baden-Powell and,—I think in our own time, Hahn—have left a school, a method, and the force of a great example.

Summary would be treacherous. Yet, looking back over the era, I think we might venture to distinguish two clear tendencies. The first is a shift and an enlargement of the educator's interest. He has come to be concerned less with content and curriculum, less with the sheer process of learning and more with the nurture and enrichment of a total personality. He has more fully in view the whole range of the unfolding powers of his pupils, and seeks that heart and hand and eye and ear, as well as mind and will, should find expansion and delight. The second is a related enlargement of our conception of the school, and a new emphasis on its corporate character. We have come to see the school not as an agency of limited utility, but as in its own way a community of citizens, working out the pattern of its own life. Here, most manifestly, we draw upon our living past, upon the experience of the residential schools, and upon the collegiate basis of the ancient Universities of England. But it is no matter of mere repetition, of trying, so far as may be, to copy the old. It is a new creation which has to discover its conditions and possibilities. A great deal of experiment is wanted—especially I should judge, to discover in what size of school this experience of community may in general be most fully realised. In a society so exposed as ours to the risk of fragmentation, this emphasis seems to me immensely important : and I am moved to hope that when we are reasonably clear as to the ' natural ' size of the school, we shall not allow considerations of administrative convenience to drive us to the creation of larger units.

III. I come to my final topic, from retrospect to prospect, an estimate of the task which lies ahead. We have looked at the lines of the achievement of a century—the making of at least the possibility of a genuine national system of education, universal and reconciling. And more than the possibility. For all the harshness of the times, these fifty years have seen impressive action : we may believe that our hope takes form and substance. But we are far from our goal. We have much to overcome, and no title yet to think of ourselves as an educated people, or firmly possessed of the measure of civilisation which we have created. In one sense so it must always be. By no contriving of ours, can we attain to the Kingdom of Heaven, or build an earthly kingdom secure against the hazards of human sin and folly. That need be no discouragement : for well short of the ideal, we can be a great deal better than we are, and release ourselves from triviality. We aim then, through our education, to make ourselves better members of a better society, more fully in possession of all that is available to us, at once more sensitive and more content. What programme promises to take us on our way ?

The answer is, I think, that no new programme is wanted—nothing more than to give reality to the one we have. We have come to the threshold of opportunity. We have still to show that we can use it. A longer school life : secondary education for all. These may be the means of a genuine gain, will be, if we employ them with understanding and imagination. Or

they may be the means of boredom and frustration : in which event, our second state will be worse than our first. It will take boldness and imagination and methods other than those with which we are familiar : for we have to meet a new constituency at the point of its own aptitudes and interests, and through relevant and intelligible disciplines give them an experience of meaningful virtues and values.

Nor are the general instruments of our task very much open to our choosing. As were the men of 1851 so are we now involved in an historic situation : and it sets its own conditions. On one side—the side of economic necessity—the requirement is altogether plain. There are 50,000,000 of us on this little island, not happily distributed in age, irretrievably committed to an urban, industrial way of life, in need of food and raw materials which can come only from other lands, and from peoples whose demands upon their own resources are mounting every day. We have no claim now to the dividends on past investments. These have gone. All we get we shall get only in return for the fruits of our own labour of hand and brain, which must make their way in a world market less easily accessible to our products than at any time in our history. No question, therefore, but that we must make the utmost of our resources of invention, of energy and of technical skill. Better technology, better leadership, better administration, and therefore better instruction and more research in all the relevant sciences, whether of natural phenomena or of human behaviour, and better habituation in applying knowledge to practice—all this is a primary and inescapable demand. Much of our longer educational life must go into meeting that requirement.

Yet, comparatively, that is the easy part of our problem. For, as to techniques we can certainly, by taking thought, add a cubit to our stature. There is a further and harder call, part challenge, part opportunity. It springs from this—that technical efficiency alone gives no assurance that even economically a society will survive, still less that its members will have a worthy personal and social life. If that is to happen, men must learn with steady faithfulness to maintain together the intricate and delicate relationships upon which a modern society depends : and that in turn requires that they must find or create within that society and in all that grows out of it, an experience which in some sort gives meaning and purpose to their lives, with which they are content.

That is the deepest need of all. A community is not secure until its members feel that as their society calls upon their service, so, as far as may be, in a measure not widely disproportioned to what they give, it opens to them access to those interests and activities which are the constituents of a good life. ' Such,' said Pericles in praise of Athens, ' is the city for whose sake these men nobly fought and died : they could not bear the thought that she might be taken from them ; and everyone of us who survive should gladly toil on her behalf.'

So, as well as efficiency, we must learn what these interests are and prepare ourselves for their enjoyment. In part they are the familiar interests of home and friends and work, and leisure and readable books and children who look better than their parents. But familiar as they are, and the strong outgrowths of natural affection, they cannot be attained on a natural basis alone. They imply perceptions and restraints which call for disciplined judgment—in some sort for the educated mind. We are still far from the assurance that all our people are fit to find their satisfactions there or are ready to accept the responsibilities which they impose. Even at this level, there is a large and relevant work for the schools to do in the later years when the interests of adult life are stirring. They have a chance to shape a scale of values, and to help the young to discover where the rewarding experiences lie. Of course the schools alone cannot finish the task. Let us not forget that there are other agencies of education than the schools and colleges : that indeed these will succeed best in their task if, as their pupils and students pass from them, they are expectant of the more that will come with the experience of maturer years. The best fruits of learning are slow to ripen and are gathered in later life. Yet the part of the schools is

all but indispensable. That is the time of preparation.

To this end, as I think, there is one sovereign rule. Again it is no new discovery. It is indeed the open secret of the best practice of our long education. The great schools of Oxford and Cambridge formed the taste and judgment of generations of men not because they taught the literature of the ancient world, but because that literature nobly expressed the experience of a noble civilisation. For a space men lived with the masters : and the masters set the mark of greatness in their minds. That is the element which somehow has to find its place in our provision.

The classics, I doubt not, retain their ancient power. But in our present context, they are too costly to help us much. Happily they have no monopoly. There are other and more open roads, so long always as we secure that they lead to the experience of the excellent. I have known men of my own land, mostly alas ! elderly, and most of them country-bred, of no great schooling and no wide accomplishment, who were yet, and enviably, educated men. They knew the ways of Nature, and they knew their Bibles. They had no rubbish in their minds ; they thought and spoke with wisdom and dignity.

Exposure to the best. That is the way to the possession of standards and to a scale of values. Let us where we can use the way of the great books : there will be time for reading in the longer curriculum. But we have to seek it too, in other ways, especially through the discipline of practical learning. For that is the very heart of the challenge with which as educators we are faced. We have to engender not only the skill and the will to do, but the conviction that the doing is itself rewarding. Whatever the instrument—solving equations, judging the quality of timbers, using a lathe, playing a fiddle—we miss the summit of it all if there be no moment when the mind of the learner takes fire, when he has sight of a kind of perfection attainable in this activity, so that here at least he has hold of an ideal standard, and wants with all his heart to do this as well as it can be done.

By that way comes integrity of judgment which is an element in integrity of character. I know no other. No matter that a man cannot himself be a master. Few of us can, in anything. But he knows the difference between the first-rate and the second-rate : he knows what excellence is, that it is worth having, that it is costly and not to be had for the asking : and he is by that the less at the mercy of the abounding cheap and the shoddy.

I do not think that this is easy : I know it is not easy, even in the favourable conditions of a University. I know that it cannot be quickly done, or ever universally. There are formidable forces in the way—bad housing and grievous industrial squalor, much of our current literature and entertainment that lives on our vulgarity and want of taste : above all our own lethargy of spirit of which these things are the outcome. Not long ago, Sir William Haley remarked that the one certain way for the B.B.C. to increase its audience was to lower its standards. That is the measure of what we have to do.

But however hard, however distant, this ambition somewhere to arouse an apprehension of the best still remains the thing most worth the winning. It must stand at the centre of our educational effort. And we have right to believe that for all its tardiness and frequent failure, the response is still effectual. We may be few ' capacious souls, placed on this earth to love and understand.' But there is in most a spark that may be kindled : and our present poverty is due at least in part to the lack of those opportunities which now come into sight. If you doubt it, consider this. Look to the quality of that growing number of our citizens who over a generation have had a chance of secondary education. Is it open to question that there is a higher level of intelligence, taste and responsibility ? I do not think so. Or, and this is as near as may be, an *experimentum crucis*, consider the B.B.C. It is and must be a large provider of entertainment. It might have been little more. But from the beginning, it has accepted a deliberately educational aim, and quite certainly it has not failed. In many ways, especially in its natural field of music, it has enriched and deepened the experience not of the few, but of a great multitude.

Hard it is : but in a measure, perhaps in increasing measure, it can be done.

I add one final word. I have said that we are involved in a historic situation. I need not remind you that that is true not of ourselves only, but of other countries as well, and that in one of its aspects this mutual involvement wears a threatening face. We cannot now unravel the tangle of motives which have brought us to this predicament. But amid the turmoil of those reviving fears, resentments and ambitions, one issue, I think, is plain. This tension is but a further stage of the conflict between two different conceptions of human society. We have our own, the impulse of which I have called liberality. Slowly and with much faltering, the Western world has come to think that the best society is that which acknowledges the rights of its members as persons, which therefore founds its action on their responsible participation, and accepts the judgment of their will and conscience. Its purpose is the making of persons : it must therefore take the risks of leaving room for personal choice and personal action : and its achievement is the measure in which by their association with one another, and in response to the living spirit of their community, its citizens win their way to a life of dignity and satisfaction.

There is another, a totalitarian view—its precise colour makes little difference—which holds this hope to be illusion. Ordinary men are not fit for such a destiny, nor do they greatly desire it. They want a tolerable time, the sense that they are fairly done by, and the prospect of an improving material estate. Let them have that, and they are content, as they should be, to forgo the experience of political responsibility, and to leave the ordering of things to their appointed leaders. So what matters is not the making of persons, but the security of citizens within a firmly organised society, in such privileges as the State decrees. The State, you see, is itself the judge, not subject to judgment : and the supreme objective, the one criterion which is everywhere relevant, is the mystique and ongoing of the State itself. That is what it has to come to : perhaps unexpectedly to some of its more philosophical proponents ; but assuredly in practice and in the directing minds. And it has a powerful appeal. In a perplexing world, it provides a creed, it lifts from the individual the burden of doubt and of making a genuine personal decision : it gives him an assigned job to do, and the deep emotional satisfaction of absorption into the biological mass of his own collectivity, moving, as it seems, to a destiny of its own. The only price he pays, unless he be one of the few who can climb to power, is that he lets himself go, and turns away from the painful labour by which selfhood may be won.

I do not know, nor does anyone, how and when the resolution of this antinomy will come, whether without calamity our civilisation can overcome the doubts and divisions in its own mind. But I am sure that here and now we have to settle where we mean to stand, for that decision affects all our planning, in Education as in all else. The totalitarian pattern sets a simpler educational problem : and can count on an easier short-term success. The alternative, our historic way, aims at a wider and longer objective. It must use a greater diversity of means and doctrine and therefore incur the greater risks. Yet, for us, I hope, the decision is clear, has indeed been taken. We have planned for a liberal society, and have laid our educational lines. We have no cause to retrace our steps. Our century's effort and much of our effort for long before, is but a part, a formative part, of that movement in our Western World which has inspired its noblest art and its most generous action. It may be illusion, as our critics say. It is certainly an act of faith. But this at least is true—that our educational system, as increasingly it has expressed this impulse, has been of power in the healing of our own society. Perhaps, then, we have some right to think that the same spirit may be of power in the healing of all the nations.

EXPERIMENTAL AGRICULTURE

Address by

DR. E. M. CROWTHER

PRESIDENT OF SECTION M

I PROPOSE to review some of the main steps in the development of experimental agriculture during the last hundred years, choosing my examples from the study of soil fertility and manuring. I shall leave out most of those branches of agricultural science which may equally be regarded as agricultural applications of the other sciences represented in this Association. Whilst confining myself to one part of a subject undoubtedly belonging to Section M, I shall try to show how experimental agriculture has built up its own rigid discipline.

The middle of the nineteenth century—the Golden Age of British Farming—forms a convenient starting point, because at that time farming in this country was passing with almost revolutionary speed from the subsistence to the commercial pattern in order to feed the rapidly growing urban populations. The Agricultural Societies were recording the results of many new enterprises and encouraging their members to undertake and to report on experiments made on their own farms.

In Edinburgh it is fitting to recall that the first critical review of field experimentation grew out of work initiated here in 1842 by a group of Midlothian farmers, with Mr. Finnie of Swanston as the first mover. Originally it had been proposed that the Highland and Agricultural Society should appoint a chemist 'who on very reasonable terms, should analyse soils and manures, and probably give lectures.' The Society, however, 'had not leisure and funds for the project,' but they gave every encouragement to forming the Agricultural Chemistry Association of Scotland, to be financed by subscriptions. The Objects of the Association include the following statement:

'The great design of the Association being the improvement of Agriculture by the Application of Chemistry, Vegetable Physiology, and Geology, it contemplates as its leading objects—

'1. The diffusion of existing information. . . .

'2. The enlargement of our present knowledge, by experimental investigations of practical Agriculturists in the field, and of the Chemist in the laboratory. . . .

'The enlargement of our present knowledge, by experiments in the field and laboratory, is what must be regarded the more important purpose, and it as is that which will chiefly engage the chemist.'

Professor James F. W. Johnston of Durham was appointed Chemist in July 1843. His investigations and writings, from which I shall quote, with his teaching and travels did much to influence the development of agricultural science in this country and abroad. One of his American students, J. P. Norton, who won a Highland and Agricultural Society Premium of Fifty Sovereigns for an interesting paper on 'The analysis of the oat,' on his return to Yale in 1846, became the first Professor of Scientific Agriculture and set up the School of Applied Chemistry. His successor, S. W. Johnson, has recorded that the Connecticut Agricultural Experiment Station, founded in 1877, 'was the first permanent organisation of the kind in America and has largely grown out of the influence of Professor John Pitkin Norton, a pupil of Johnston in Edinburgh and Mulder in Utrecht.' It is pleasing to follow the indirect influence of Midlothian farmers on the development of agricultural science in the United States.

The analysis of soil fertility problems

At the present time we are again in a period of rapid technological and economic changes with an expanding demand for agricultural products and increasing costs of production. In many parts of the world agricultural pioneers, who now are often the officers of Agricultural Departments, Research Institutes or Corporations, are trying to forecast how soils and crops will behave under new agricultural systems, for which there is little or no local traditional experience.

It is axiomatic that new developments must safeguard the productivity of the land or, in other words, that soil fertility must be maintained or, still better, enhanced. Difficulties often arise in applying this maxim in specific cases. Soil fertility depends on three main groups of factors : those inherent characteristics of the soil profile which are studied and mapped in soil surveys, the local weather conditions and the recent agricultural operations. The behaviour of soil water and the conservation of the surface soil are of paramount importance. Where these are neglected, the results may be disastrous, as was seen some twenty-five years ago when the Uganda Department of Agriculture set out to demonstrate the merits of clean cultivation by ploughing. Undue exposure led to soil erosion, the yields rapidly fell and in a few years nothing was left but a barren ironstone pavement. If soil moisture and erosion are controlled, an inherently poor soil may be made highly productive. Some of the most valuable soils in the world—the hyacinth bulb soils of the Netherlands—consist of almost pure quartz from sand dunes, levelled to precisely 55 cm. from a permanent water-table. There are several other places, e.g. in Florida, where soils serve mainly to give root-space for plants grown in nutrient solutions. In the rest of this address I shall deal with more ordinary soils and shall omit special methods of soil conservation and reclamation.

Some practical men and administrators, and perhaps even some members of other Sections of the British Association, may imagine that the principles of soil fertility can be derived deductively from the pure sciences. Many members of this Section will prefer to proceed through generalisations built up from a body of carefully ascertained facts—through what Johnston called ' inductive experimental agriculture.' The contrast between the deductive and the inductive approaches to soil fertility problems can be illustrated from the writings of the main protagonists of the two schools a century ago. In the preface to the third edition (1843) of his ' Organic Chemistry in its applications to Agriculture and Physiology,' originally prepared for this Association, Liebig wrote ' Now that the conditions which render the soil productive and capable of affording support to plants are ascertained, it cannot well be denied that from chemistry alone all further progress in agriculture is to be expected.' Lawes replied in 1847 that ' Agriculture will eventually derive the most important assistance from chemistry, but before it can propose any changes in the established routine of the farmer, it must, by a series of laborious and costly experiments, explain this routine in a satisfactory manner.' Again, in 1851 Lawes and Gilbert wrote : ' If then we would attain by the aid of science a rational system of agriculture the actual facts of the art itself, as well as the indications of direct experiments in the field and the study of the functional actions of plants and animals, must receive a due share of attention. In fact Chemistry alone will do nothing for practical agriculture.'

Some problems in soil fertility may fall into better perspective if we recall that the basic discoveries in agricultural practice were made in prehistoric times. Examples are the selection of the main species of cultivated crops and domesticated animals, the establishment of crop-rotational systems, the chalking of land in Britain and irrigation in the Middle East. Primitive cultivators in Africa and Asia know that land needs to be rested after spells under tillage. Thoughtful men at many times and in many places, must have wondered what precisely was ' worn-out ' by tillage and restored by ' resting.' The pattern of alternating cultivation and resting can be traced through history—and matched across continents—from shifting cultivation, through the wild grass-wheat rotation, the medieval three-field system and the Norfolk four-course

rotation to the Scottish system of long leys. The details have changed but the basic rhythm remains. In industrialised countries additional long-term cycles have been superimposed as ploughing up campaigns have been followed by depressions in which land has tumbled back to grass.

The nutrient elements

The early natural philosophers, such as von Helmont and Robert Boyle, grew plants experimentally to analyse air and water, but there could be no real progress in interpreting plant growth or soil fertility until the basic principles of chemistry and plant physiology had been established. Early in the nineteenth century Sir Humphry Davy and many other authoritative writers accepted the traditional view that plants feed on humus in much the same way as animals feed on plants. The demonstration by Senebier and de Saussure that plants can obtain most, if not all, of their carbon from the atmosphere was profoundly disturbing to the experts of the time. Liebig's great service to agriculture was to stimulate discussion and controversy by expounding the new interpretation and ridiculing the old one, but his writings provide many examples of the danger of approaching agricultural questions through chemical theorising unchecked by field trials. The foundations of soil science were laid by Boussingault in France and Lawes and Gilbert in England, who concentrated on establishing the facts by careful experiments in field plots and pots, with chemical analyses on the soils, manures and crops.

Lawes and Gilbert soon demonstrated the striking effects of ammonium salts as fertilisers, and as early as 1846 Lawes had concluded that 'Until chemistry has solved the great problem of uniting hydrogen with nitrogen, and can furnish us with a cheap and abundant supply of ammonia, the employment of artificial manures must be confined to one or two crops.' This remarkable forecast had to wait seventy years for its practical fulfilment.

Liebig, on the other hand, was convinced that it was impossible to make significant quantities of ammonium salts and that in any event they were unnecessary. Even in 1863 he wrote: 'Though manufacturers of manures add a certain amount of ammonia to their productions, this is chiefly to humour the fancy of farmers for this substance. . . . This prejudice will soon disappear of itself when farmers have learnt to make a proper use of the nitrogenous food which nature supplies spontaneously to the land without aid on their part.'

For nearly fifty years the most difficult theoretical problem was to explain the beneficial effects of legumes on following cereal crops, as described in Vergil's famous lines in the *Georgics*. Boussingault had found that clover could obtain nitrogen from the air, but, in a pot experiment with extreme precautions against contamination, Lawes, Gilbert and Pugh failed to confirm this. In 1880 Hellriegel and Wilfarth made the critical observation that in an irregular pot experiment the good clover plants had root nodules, which were subsequently shown to contain micro-organisms capable of absorbing atmospheric nitrogen, and passing it to the host plant. Later, it was found that the other micro-organisms living freely in the soil could also fix nitrogen.

This brief summary of a long enquiry into the source of nitrogen for plants will serve to focus attention on two points still of the greatest practical importance. The potential use of nitrogen fertilisers is limited by progress in chemical technology and by the skill of agriculturists in using nitrogen fixed by micro-organisms or in factories. On the grasslands of Britain and the prairies of North America and in much of the tropics, a proper choice between legumes and fertilisers can be made only after prolonged trials in the field.

The value of bones had been known for centuries but the results were peculiarly erratic and uncertain. By introducing superphosphate made from sulphuric acid and bones or phosphate rock, Lawes in 1842 provided a much more active and reliable material, which still forms the basis of the fertiliser industry. Over many decades the world consumption of phosphate rock, mainly as superphosphate, continued to increase at roughly 7 per cent. per annum. This may represent approximately the rate of growth of knowledge in the use of fertilisers. There

are good grounds for expecting continued progress at a similar rate, for there are abundant reserves of phosphate rock and, hitherto, almost negligible quantities of fertilisers have been used on the inherently poor soils of most of Africa, Asia and South America.

Lawes and Gilbert soon found from their field experiments that chemical analyses did not reveal the nutrient requirements of different crops. Thus, turnips contain relatively small proportions of phosphorus but respond well to that added in superphosphate. In his mineral theory Liebig had stressed the importance of farmyard manure as the main source of the mineral elements and as the principal agent in disseminating them in useful forms throughout the soil. He did not, however, try to assess the relative importance of the various elements present and he poked fun at the amateur attempts of Lawes and Gilbert to do so. The practical value of potassium salts was not established until the 1880's, when commercial supplies became available from the German mines and were tested on many farms.

The attitude of farmers and agricultural chemists to calcium and magnesium limestones illustrates in a peculiar way the danger of relying on a few experiments at isolated centres. The use of burnt lime or chalk was traditional in Britain; Johnston had written a useful text-book on the subject in 1849. The practice fell into decay in the latter half of the century as more immediate and profitable results were obtained from fertilisers. Lawes and Gilbert paid little attention to liming because most of their Rothamsted fields contained large residues of calcium carbonate from earlier chalkings. The science and practice of agriculture might have taken a different course if the estate inherited by Lawes had been on a light sandy soil, or, alternatively, if an Agricultural Society or some other landowner had been enterprising enough to repeat the Rothamsted experiments on such land. This had to wait until 1876 when the Duke of Bedford and the Royal Agricultural Society set up the Woburn Experiment Station. Within twenty years continuous barley crops failed disastrously on plots receiving ammonium sulphate. It was found that the soils had become acid to litmus paper and that good crops could be grown again after liming. These results and similar ones in the United States at about the same time renewed the practical interest in liming and stimulated chemists into devising a variety of tests for soil acidity and lime requirements. Measurements of *p*H values provide the advisory officer with one of his most valuable aids but, in spite of a great deal of propaganda and large subsidies, shortage of lime is still one of the most serious factors limiting crop production in this country and in several others with humid temperate climates.

British farmers were slow to appreciate the merits of ground limestone, though the results of the few comparative experiments suggest that it is slightly more effective than equivalent amounts of burnt lime. It is certainly more convenient to apply.

There had also been a prejudice against the use of magnesian or dolomite limestone, which is still enshrined in the Regulations of the Fertiliser and Feeding Stuffs Act of 1926. Fortunately the Agricultural Lime Division of the Ministry of Agriculture was persuaded to pay the usual subsidies on magnesian limestone in spite of these Regulations, which it is hoped may soon be revised. The prejudice may have survived from some very early experiments in which heavy dressings of burnt magnesian lime proved too caustic. The occasional use of magnesian limestone or soluble magnesium salts may reduce the risk of magnesium deficiency in sensitive crops, especially in light soils heavily dressed with other fertilisers.

Sodium too had a chequered history. Some of the striking effects of sodium salts on sugar beet and mangolds at Rothamsted were ascribed to the liberation of available potassium within the soil. During the last ten years or so some 200 experiments on sugar beet have shown that sodium greatly increases yields, 5 cwt. of salt per acre giving on the average 5 cwt. of additional sugar per acre. The sodium is absorbed in large quantities but there is no increase in the amount of potassium taken up by the crop. Sugar beet and mangolds are the only common farm crops known to respond to sodium in this way.

It has been known for many years that several other elements are essential for plant growth in water or sand, but for a long time it appeared that most soils supplied these elements in sufficient quantities. Systematic search has led to the discovery of conditions in which crops can be cured of chlorosis and other pathological symptoms by supplying small amounts of appropriate salts. There were many striking examples of deficiencies of manganese and other elements in the United Kingdom during the war, when untried land was brought under cultivation, often with heavy liming. Deficiencies are often found in fruit trees and vegetables. Sometimes livestock suffer from a deficiency of cobalt, though the herbage may appear normal. In parts of Australia good pastures have been established for the first time by supplying salts of copper, zinc or molybdenum, the latter at rates of only a few ounces per acre.

The question has often been asked whether small amounts of these ' micronutrients ' or ' trace-elements ' should be included in ordinary fertilisers to cover the risk of unrecognised deficiencies. At present it seems wiser to await definite evidence of the need, because excesses are always wasteful and may even be dangerous. Although ' heart-rot ' in sugar beet, caused by deficiency of boron, may occasionally damage crops, the average results of experiments on over a hundred farms in six years failed to show any difference between plots with and without added borax.

Practical tests

Early writers on ' Experimental Agriculture,' such as Arthur Young, regarded an experiment as a practical test of a single treatment. They gave detailed costings of all the operations, including the cost of beer for the harvest, to show whether the venture was a practical success. Data for isolated fields or comparisons between neighbouring farms are, however, treacherously difficult to interpret because it is rarely possible to isolate the relevant factors. Newer techniques are generally first taken up by the more enterprising farmers, who are skilful in making timely adjustments to changing conditions of all kinds. Dr. Yates will be dealing this morning with the special problem of analysing data from agricultural surveys.

Simple trials with a few plots or an untreated strip may provide useful local guidance on matters established elsewhere. The relationship between preliminary tests on a few plots to more critical experiments is sometimes misunderstood. It has often been said in discussions on tropical research programmes and it has even been whispered in connection with the new Experimental Husbandry Farms in England and Wales, that very simple trials will suffice where the immediate object is to solve practical problems and not to do academic research. This attitude overlooks the point that locking-up limited resources in inadequate trials may postpone attacks by better methods or, what is worse, may lead to erroneous conclusions requiring years of exacting work to correct.

Preliminary trials, sometimes on a vast scale, may be needed to decide the technical feasibility or desirability of a treatment under consideration for more detailed investigation. Thus, there would be little point in embarking on elaborate rotation experiments in Central Africa until it is known whether the proposed cropping systems will in fact work. It is doubtful whether detailed assessments of the manurial value of liquid manure, stored and applied in the Danish way, would be worth while in the United Kingdom or the United States until the system is shown to fit satisfactorily into local conditions.

Early field experiments

In his book *Experimental Agriculture, being the results of past and suggestions for future experiments in scientific and practical agriculture*, published in Edinburgh in 1849, James F. W. Johnston was forced to the conclusion that ' scarcely any of the results we have as yet obtained are to be relied upon as secure grounds for scientific opinion ; yet they may be considered to have cleared the path to surer results by pointing out sources of error previously unknown, and thus indicating the precautions which must be adopted in future trials.'

Johnston realised that no statement of comparative yields was of any value unless it was accompanied by some indication of the variation between similarly treated

plots scattered through the experimental area. He raised, but could not answer, the important question how far discrepancies between yields on similarly treated plots affect the degree of confidence to be attached to differences between mean yields for contrasted treatments. A sound and convenient answer to this question was not given till seventy-five years later. Johnston also saw the advantages of comparing manurial treatments both singly and in combination, for the effect of one treatment may be modified by the presence of another treatment.

It is not easy to understand why the warnings and advice of Johnston and his immediate successor, T. Anderson, had so little influence on the subsequent development of agricultural experimentation. Johnston may have advanced too bewildering a variety of unrelated proposals. Farmers may have been discouraged by the contradictory results on different soils. At that time chemists appeared to be expecting all soils to respond in much the same way to various kinds of fertiliser. Thus, in 1880 when T. Jamieson in Aberdeenshire reported almost as good results on turnips with finely ground mineral phosphates as with superphosphate, he asserted that this would cause a complete revolution in the manufacture of artificial manures. A. Voelcker in England replied by ' directing attention to a few published experiments which are diametrically opposed in their results to those which Mr. Jamieson has obtained, and which conclusively prove the economy and beneficial effects of dissolved phosphate manures.' It was not until fifty years later that both kinds of results, debated in the ' Battle of the Phosphates,' were obtained in the same investigation, with the important addition that it was possible to relate the observed differences in behaviour to properties of the soils tested. Ground mineral phosphate often gives good results on very acid soils, such as Jamieson had used in Scotland, but not on slightly acid or neutral soils, such as Voelcker had used in England.

An important discussion on ' The Interpretation of the Results of Agricultural Experiments ' was held at the British Association meeting in 1911. Speaking of County Trials with single plots for each treatment, T. B. Wood said ' Half the reports of such trials consist of explanations of the discrepancies between the results obtained and the results which should have been obtained. The moral is obvious. The single-plot method fails signally when the subject of investigation is with differences of 10 per cent.' But 10 per cent. differences may be of great practical value.

The advantages of replicating treatments from three to six times in plots of about one-fortieth of an acre were realised by P. Wagner in Germany in the 1880's. The replicates were arranged in various systematic patterns, similar to those devised by Johnston. Series of experiments on large numbers of farms were laid out on these lines by the Danish co-operatives and, later, by the German ' Experiment Rings ' and the Swedish State Agricultural Officers, the expenses being met by the farmers. Central laboratories correlated the individual results with laboratory tests on the soils.

Modern field experiments

Rothamsted was still laying out single-plot experiments in the early 1920's, but it atoned for its shortcomings by stimulating its statistician, R. A. Fisher, into devising new experimental methods in the light of his mathematical researches into variations in small samples. He showed that ' the difference between plots chosen as parallels must be truly representative of the differences between plots with different treatments ; and we cannot assume that this is the case if our plots have been chosen in any way according to a prearranged system.' Although various restrictions may be imposed, the actual assignment of treatments to plots must be at random. Fisher solved Johnston's problem of estimating the reliability of observed treatment-differences in terms of the variations between replicates.

It is not always realised that in well-conducted field experiments as many as ten replicates are generally required to establish yield differences of the order of 5 to 10 per cent. This means that many plots will be needed to test whether a given nutrient improves yields and that quite elaborate experiments will be required to decide the best form, rate and

manner of applying it. Isolated experiments testing miscellaneous treatments with moderate replication are foredoomed to failure. If only a few experiments are made, one or two may happen to show chance variations in the expected direction but it would be misleading to single these out for demonstration or more detailed work.

Many ingenious methods have been devised by R. A. Fisher and F. Yates to extend the range of questions which can be studied in field experiments with between, say, 25 and 75 plots. Johnston's method of testing treatments 'with each taken singly and with the mixture which contains them' was developed systematically to provide the 'Factorial Experiment,' one of the most useful patterns now available. In a famous paper on 'The Arrangement of Field Experiments,' published in 1926, R. A. Fisher wrote 'No aphorism is more frequently repeated in connection with field trials than that we must ask Nature few questions, or, ideally, one question at a time. The writer is convinced that this view is wholly mistaken: Nature, he suggests, will best respond to a logical and carefully thought-out questionnaire—indeed, if we ask her a single question, she will often refuse to answer until some other topic has been discussed.'

In a factorial experiment testing three materials, say nitrogen, phosphorus and potassium fertilisers, every plot is used to measure the effect of each material. It happens very often that such an experiment is as accurate and informative as three experiments, each on the same scale, testing the separate elements. In addition, the factorial experiment will show how far one treatment affects the behaviour of another. Thus, farmyard manure reduces the gains from phosphorus and potassium fertilisers, but, somewhat unexpectedly, does not appreciably affect the gain from nitrogen fertiliser. Nitrogen and potassium fertilisers used together give more than the sum of their separate effects on sugar beet, but not on potatoes.

Sometimes such so-called 'interactions' supply useful information about the way various treatments act. Sometimes they point to favourable combinations of treatments leading to improved systems of crop management. An important early example was an experiment by F. G. Gregory, F. Crowther and A. R. Lambert in 1929–30 on irrigated cotton in the Sudan Gezira. The earliest sowings gave the biggest response to nitrogen fertiliser; the second sowings gave the best average yields; the third sowings could be improved only by close spacing. Extra water, an important factor in an irrigation scheme, improved yields only where cotton was sown early and manured. These findings, which were confirmed in later years and in other centres, played a large part in the notable success of the Gezira Irrigation Scheme.

In many parts of the world one of the best approaches to soil fertility problems would be to test all combinations of nitrogen, phosphorus, potassium fertilisers and two other factors, e.g. farmyard manure and limestone. This can be done conveniently on thirty-two plots, set out in four blocks.

The most profitable dressings within the normal practical range can often be assessed sufficiently well by experiments with three rates of application, e.g. 0, 1, 2, preferably testing two or three nutrient elements in a single experiment. Field experiments are rarely precise enough to determine the exact form of relationship between yields and rates of treatment, but simple expressions for the Law of Diminishing Returns can be used for calculating responses for other rates near those actually tested. It may, however, be very dangerous to extrapolate far beyond the experimental range using arbitrary constants, as has been done by E. A. Mitscherlich and O. W. Willcox for nitrogen.

Although it is often most informative to follow by measurements, samplings and analyses the progressive development of the test crops, it is unwise to judge the results of treatments solely from the early stages. Thus, soluble phosphorus fertilisers placed near the seeds of many crops stimulate early growth. This may be of great importance in dry regions and for quickly maturing crops. It may also account for the continued heavy use of phosphorus fertilisers on soils already well supplied with this element. On many

soils and crops, however, the early stimulus wears off as the season advances and does not show in the final yields. Observations made only in the early stages may, therefore, be misleading, whether they are made visually or more elaborately and expensively by measuring what proportion of the phosphorus in the plant came from a fertiliser tagged with radioactive phosphorus.

Wherever possible, experiments should be conducted at several centres and in each of several seasons to build up more representative averages. The need for large series of experiments means that most short-term experiments on soil fertility questions must be conducted on ordinary commercial farms, for there can never be enough experimental farms to provide sufficient variety of soil conditions. The experimental farms should confine their work to long-term experiments requiring security of tenure and to short-term experiments involving operations too complicated or observations too detailed to be carried out at a distance. The experimental farms must also hold some of their most uniform fields at low nutrient levels if, later on, they are to be suitable for soil fertility experiments. This requirement is difficult to reconcile with the desire of farm managers and committees to show bumper crops and favourable balance sheets. There is a strong case for discriminating sharply between demonstration farms and experimental farms. It is still not quite certain into which class the new Experimental Husbandry Farms in England and Wales will fall.

All possible methods should be used to characterise the soil and the general agricultural conditions in each experiment so that, as the series grows, the results may be classified into various well-defined groups or related to soil data by regression analyses. For over a century farmers and chemists have hoped that the principal factors in soil fertility could be determined by soil analysis. Since Daubeny's Bakerian lecture of 1845 it has been known that a distinction must be made between what he termed 'the *dormant* and the *active* ingredients of a soil with reference to the plants which grow on it.' A good deal of confusion is still caused by the custom of expressing the results of soil analyses as 'available plant foods.' There are no sharp distinctions between the various kinds of phosphorus and potassium compounds in soils, and there is no reason to assume that any analytical separation will pick out the forms plants use. The methods of soil analysis used in advisory work are empirical but some of them have proved very useful. It is one of the main functions of series of liming and manuring experiments to provide data for calibrating and improving these methods.

There is no need to postpone the field trials until the soils of the region have been mapped, for, if a uniform site representing an appreciable area of land is chosen and its position recorded by grid-reference, the results of each field experiment may later be of service to soil surveyors in the practical application of their work. One of the main difficulties in the highly developed Soil Survey of the United States is to obtain sufficient reliable field data for individual Soil Series and Phases for making productivity ratings, economic analyses and advisory recommendations.

Fertilisers

Every farmer who uses manures or fertilisers must make up his mind how to allot them between his various fields. The rules for rationing feeding-stuffs to stock are clearly formulated and widely used, but those for rationing nutrient elements to soils and crops are much less understood. When a fertiliser policy had to be worked out on a national scale during the war, E. M. Crowther and F. Yates showed in 1941 how the results of past experiments on fertilisers in the United Kingdom and Northern Europe could be used to provide an efficient means for adjusting fertiliser practice to varying supplies of raw materials and priorities for crops. The principles they advocated were embodied in the Fertiliser Rationing Scheme for individual farms, so admirably organised by the Fertiliser Controller, Mr. H. U. Cunningham, the third speaker in this morning's session. Similar methods may be applied far more generally. The first step would be to obtain the average responses of each crop to standard amounts of nutrient elements, and then to evaluate the most profitable rates for current economic conditions. The next step is to make

adjustments for local soils and conditions. Thus, less nitrogen and more phosphorus would be required in wetter regions, and less phosphorus and potassium on the dunged fields. Up-to-date recommendations on these lines might become part of the advisory services of official bodies or farmers' organisations, when the necessary field experiments have been made.

How much remains to be done in relating fertiliser practice more closely to soil conditions may be seen by noting some of the variations between neighbouring countries. In both the United Kingdom and the United States, where fertiliser consumption has almost trebled in the last ten years, the elements N, P and K are used in roughly similar proportions, about 200,000 tons of each in the United Kingdom and about 1,000,000 tons of each in the United States. In the Netherlands, which uses fertilisers about twice as heavily as the United Kingdom, the N : P : K ratio is about 1·0 : 0·3 : 0·8 and in Germany it is about 1·0 : 0·5 : 1·5. At first sight these variations in practice might be taken to indicate inherent differences in the soils and general agricultural conditions, but field experiments in several Western European countries show very similar average responses to unit amounts of N, P and K in fertilisers. The national differences in fertiliser practice must depend largely on historical and commercial factors.

Similar differences are found in the principal kinds of fertiliser employed. Thus, nitrogen is used mainly as ammonium sulphate in the United Kingdom, as ammonium nitrate (mixed with calcium carbonate) in the Netherlands and as calcium nitrate in Denmark. In the United Kingdom and the United States most fertilisers are used in mixtures, but little or no mixed fertiliser is sold in the Netherlands or Denmark. In the United Kingdom the mixed fertilisers are mainly in granular form and almost all of the phosphorus is in water-soluble compounds. In the United States most of the mixed fertilisers are sold as powders with less than half of their phosphorus in water-soluble forms. Anhydrous ammonia and ammonium nitrate are produced more cheaply than ammonium sulphate in the United States and they can be included in mixed fertilisers only by reducing the solubility of the phosphates present.

There are many alternative kinds of phosphorus fertilisers on the market or at the pilot plant stage of development. The current sulphur shortage gives particular interest to those made without sulphuric acid, e.g. concentrated fertilisers made from phosphate rock, nitric acid and ammonia. On very acid soils finely ground untreated rock phosphate may be economical, especially if used to build up reserves by heavy occasional dressings with small amounts of a soluble phosphorus fertiliser drilled with the seeds of most crops as a ' starter.'

Much careful experimental work will be needed before the value of some of the newer forms can be properly assessed and fitted to the most appropriate conditions. In the past many fertilisers of low efficiency were overrated because they were tested in the field only at heavy rates, giving yields on the flat and unresponsive part of the curve. All comparative trials on kinds of fertiliser or liming material should be made at relatively low rates of application, and at least one of the materials should be used at two different rates to indicate the sensitivity of the experiment. G. W. Cooke and I have applied a standard material at 0, 1, 2 units and two or three others at 1·5 units.

Very few experiments have been made to measure the rate at which fertiliser effects fall off over years. It is therefore difficult to estimate compensation for unexhausted manurial values or to make practical use of laboratory investigations on the chemical behaviour of fertiliser residues in the soil. Fertilisers are commonly applied close to the seeds but the residues are dispersed throughout the soil by ploughing. As residual effects are generally small, farmers are advised to give frequent light dressings and to regard the soil as a poor storehouse for nutrients. Yet, landowners often insist that heavy manuring has lasting benefits. This has recently been illustrated by a continuous barley experiment at Rothamsted in which striking gains are still shown from farmyard manure or superphosphate given last century with none since. The total applications had been so heavy that even a small percentage residual effect is

enough to ensure good barley crops fifty years later. Fertiliser programmes must be settled in terms of rate-of-turnover and length of investment, but it must be repeated that there is very little information about the long-term effects on different kinds of soil.

Great progress has been made in recent years in applying fertilisers in special positions near, but not too close to, the seeds. Combine fertiliser-seed drills for cereals have come into general use in Britain during the last ten years. Although some promising results have been obtained for other crops, much more research will be required before we can expect to obtain benefits from band placement approaching those for cotton and maize in the United States. We have no farm crops so widely spaced. We also need trials to know whether it is economic to spread superphosphate on the hills from aeroplanes, as is being done successfully in New Zealand.

In tropical and other areas where transport costs are high, special attention should be given to obtaining large immediate returns from small quantities of concentrated materials. Most promising results have been obtained in this way in Northern Nigeria by dropping a few granules of concentrated superphosphate into the planting holes for groundnuts. In East Africa the results of current fertiliser experiments are being awaited before the authorities can decide whether to instal a factory for treating local phosphate rock or to fix nitrogen by hydroelectric power. As a long-term policy in remote countries the possibility of shipping out highly concentrated materials, such as red phosphorus, for processing locally should be examined.

The farming of many parts of Britain has been transformed in the last dozen years or so by establishing leys with new strains of herbage plants and better manuring. This illustrates an important aspect of soil fertility investigations. One of the main uses of fertiliser is to make it possible to introduce new kinds of crops of better nutritive value. Fertilisers have relatively small effects on the chemical composition of plants when the comparisons are made on a weight basis at similar stages of development. Manuring land for suitably managed leys increases the total amount of herbage and its content of protein, calcium and phosphorus by increasing the proportion of clovers and keeping the grasses in the leafy stage. In Africa improving soil fertility will improve human nutrition, not by changing the composition and nutritive value of maize and shorghum grains but by providing more food in greater variety and, no less important, more cash crops.

Soil organic matter

Many discussions of soil fertility problems stress the contribution made by plant roots and crop residues and the rôle of the grazing animal. Under many conditions the benefits from resting tillage land under leys can be obtained economically only by using livestock to consume the herbage, but it is difficult to analyse two entirely different systems of farming without many arbitrary assumptions on costings.

In the latter half of the nineteenth century most of the additional plant nutrients brought on to British farms were in imported feeding stuffs fed to stock in covered yards. To save these and other plant nutrients from the drains and to convey them to the arable land, it was essential to tread straw into manure. The rules of good husbandry of the period restricted and penalised the sale of straw off the farm. In some quarters the rough working rule that one ton of straw gives four tons of manure is still treated as a statement of manurial equivalents, ignoring the elementary fact that most of the nutrient elements come from the feeding stuffs and not from the litter. The few experiments made with varying quantities of straw for fixed amounts of feeding stuffs consumed by the same number of animals have generally failed to show much difference in the resulting crops ; much of the straw is oxidised away before it reaches the land. Where straw is used directly it is often necessary to supply additional nitrogen to compensate for that immobilised as the straw rots. The few experiments on these subjects have shown, at least on heavy soils, that the yields of root crops may be improved by ploughing in straw or long manure, perhaps through improved soil structure and reduced winter losses by leaching.

Sometimes the main contribution of livestock to soil fertility is to collect nutrient elements from catch crops or rough grazings. During wartime discussions on the treatment of thin soils ploughed out on the Chalk Downs of South England, I suggested that the benefits commonly ascribed to consolidation by the 'Golden Hoof' of the sheep might more properly be ascribed to the potassium collected by the succession of green crops in the Wiltshire rotation and then concentrated in the surface soil in the manure of the folded sheep. Experiments showed how good barley crops could be raised by suitably placing fertilisers with high potassium contents. An extreme example of fertilisers replacing sheep occurred on the heaths of the Netherlands. For centuries large areas of heath had to be left unreclaimed to provide grazing to give enough sheep manure for small patches of arable land. With fertilisers the sheep become redundant and the whole of the heath could then be reclaimed.

In many systems of farming leguminous crops provide the main source of additional nitrogen, and there are many parts of the world in which one of the urgent practical problems is to find methods for establishing suitable legumes, both on arable land and in pastures. On the other hand, experiments have revealed cases in which it may be more profitable to rely on nitrogen fertilisers. The Woburn experiments on vetches and mustard before winter wheat showed that the nitrogen fixed by the legume was lost from the soil before the wheat could use it.

Manurial experiments on pastures have often given disappointing results. Plots improved by manuring have given poorer live-weight increases because with fixed numbers of animals they were undergrazed. Large plots inevitably mean low replication and ambiguous findings. Attempts are now being made to work out suitable techniques for small plots by sample-cuts or by weighing faeces. Until better techniques are developed we shall lack quantitative data on the management of pastures, the residual benefits from leys and the relative merits of 'alternate husbandry' and 'two-compartment farming.' In an experiment started at Woburn in 1938 the yields of test crops of potatoes and barley after three years of grazed ley or repeatedly cut lucerne are better than those after three years of arable crops, including seeds hay. The gain from the leys is about equal to that from giving farmyard manure to the potatoes after the arable crops. In a more ambitious pair of experiments now in their first full year at Rothamsted an attempt is being made to compare the effects of grazed leys, repeatedly cut grass leys, repeatedly cut lucerne and a rotation of arable crops on the yields of subsequent arable crops and, at the same time, to measure the productivity under grazing conditions of three-year leys and very long leys.

It has often been assumed that there is some critical length of ley for improving soil structure, but, for the steppes of the Soviet Union, W. R. Williams has made the interesting suggestion that there should be two patterns of alternate husbandry on the same farm. On the ridges and gently sloping land a short rest under ley should be regarded as a regrettable necessity in a sequence of tillage crops. The low-lying pastures should be broken up occasionally for a short spell of leafy fodder or industrial crops.

The classification of crops into 'soil-improving' and 'soil-exhausting' is a somewhat arbitrary expression of current local techniques. Row-crops and leys remove more nutrients than cereals, which are often regarded as 'exhausting.' Where erosion is serious, all widely spaced row crops are dangerous until they cover the soil. If erosion risks can be reduced by suitable cultivations, the gains from leguminous crops in dry areas may be offset by the large amounts of water they transpire, and in some parts of the prairies it may be better to run the soil at a lower level of organic matter and supply nitrogen in fertilisers.

It is difficult to find any general nutritional basis for explaining the benefits of crop rotation. Perhaps its main purpose is to provide better control of weeds, pests and diseases and to cover the unknown risks involved in growing the same kind of crop repeatedly.

Changing systems of land use

During the last decade or so there have been many dramatic examples in the

United Kingdom and elsewhere of reconditioning worn-out and neglected land, sometimes by new methods of management and sometimes through regional schemes of drainage or irrigation. In several parts of the world important decisions must be taken about the most profitable direction in which to invest new capital for agricultural development. Should it be in large new schemes of irrigation or bush-clearing in underpopulated regions, or would it be better to improve land in settled and often overcrowded areas? An example on a vast scale is afforded in the United States by comparing the likely returns from the Columbia Basin Project with those from liming and manuring the impoverished soils of the South-Eastern States. Tennessee has shown very vividly through its Test-demonstration Farms how the countryside can be restored to prosperity by starting with phosphorus fertilisers and pastures. North Carolina has recently given a dramatic demonstration of what can be done in a short time by a research programme based largely on factorial experiments on maize. I should like to quote a few sentences from the Foreword by R. W. Cummings to a recent Bulletin 'Fertilise Corn for Higher Yields.'

'Before the turn of the century, the average yield in the State was around 11 to 14 bushels per acre year in and year out. . . . An intensive educational programme increased the average yield to around 18 to 20 bushels per acre. There it remained for almost three decades. . . . From 1938 new hybrids capable of yielding much greater harvests were developed. An intensive programme begun in 1943 on the cultural and fertilisation requirements showed that hunger signs, long mistaken for the effects of dry weather, were due to nitrogen deficiency. With better fertilisation and soil management, closer spacing was found necessary to provide the stalks required for higher yields. Closer spacing also made weed control easier by providing plant competition. This avoided the necessity for late, deep cultivation which caused root damage. . . . A goal of a 40-bushel per acre average by 1955 seemed ambitious when suggested in 1945, but appears almost in sight now.'

Such changes cannot properly be analysed in terms of the simple Law of Diminishing Returns, which applies only to variations in one factor at a time. They amount rather to what American economists term 'production innovations' or 'complex resource combination changes.' They allow agricultural production to be conducted under what is in essence Increasing Returns and destroy the old Malthusian pessimism about the prospects of the world's food supply.

Field and laboratory research

When faced with problems requiring long and expensive experiments in the field, many individuals and official bodies are inclined to look first, as the Midlothian farmers did over a century ago, to the chemist 'who, on very reasonable terms, should analyse soils and manures.' In recent years this tendency has been accentuated by rapid progress in many pure sciences with potential applications to agriculture and, in some countries, by the need for splitting much of the agricultural research into neat units of two or four years for University theses.

Scientific concepts are often used somewhat loosely in interpreting agricultural observations, although no means have yet been found for testing their relevance. A simple example may be taken from soil microbiology. Most neutral soils contain Azotobacter which fixes nitrogen in suitable culture solutions. It is difficult, if not impossible, to say whether it contributes appreciably to the nitrogen economy of soils in the field. One recent survey suggests that it does not. Detailed chemical and microbiological investigations over long periods would be needed to decide the question but, in the meantime, there is little to be gained by using Azotobacter to interpret agricultural practices.

Many laboratory techniques are too elaborate to be applied to the large numbers of samples needed in modern field experiments and soil surveys. Agricultural research has its own version of a Principle of Indeterminacy—'The more exacting the technique, the less adequate the conditions under which it can be employed.' Sometimes the difficulty arises in the opposite direction. Physical and biochemical investigations on soil structure are held up through lack of soil

samples in which contrasted structures have been produced under experimental control in rotation experiments with different kinds and length of ley. So far the field experiments required have proved too difficult.

It is essential that field experimental work should not be wasted on matters of purely local and temporary concern, but should be planned to make the fullest use of and to provide the best raw material for investigations in other branches of agricultural science.

The problem of balancing the various lines of approach may be simplified, in research and in teaching, if it is realised that agricultural science does not seek to establish general principles from which agricultural practices may be deduced, but has a more modest aim. Agricultural science may be regarded as a search for patterns of performance to be fitted to suitable landscapes. Its starting point is current practice in the region concerned or in a similar one. The features of many environments and the effects of most agricultural operations can be classified sufficiently well for forecasting the effects of moderate changes in practice, but it is unwise to make drastic innovations without pilot schemes and detailed experiments.

I cannot summarise my argument more concisely than by quoting, as relevant to-day, the final paragraph from the introduction to Johnston's ' Experimental Agriculture,' published in Edinburgh, just over a century ago.

' It is only by means of conjoined experiments in the field, the feeding-house and the laboratory—all made with equal care, conscientiousness and precision—that scientific agriculture can hereafter be with certainty advanced. If we have been long in getting upon the right road, we ought to advance the more heartily now we have found it.'

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